The Lumleian Lectures

ON CERTAIN POINTS IN THE

ÆTIOLOGY OF DISEASE

Delivered before the Royal College of Physicians, 1892

TO WHICH IS ADDED

THE HARVEIAN ORATION

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WITH A BIOGRAPHICAL NOTICE OF HARVEY

AND AN

APPENDIX OF STATISTICAL TABLES

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PREFACE

The following Lumleian Lectures are reprinted from the report in the 'Lancet,' with the addition of certain portions, particularly in the third lecture, of which time did not allow the delivery.

I have added to the Harveian Oration, by the liberal permission of Messrs. Adam and Charles Black, a Memoir of the Life and Works of Harvey, which I contributed to the ninth edition of the 'Encyclopædia Britannica.'

The Appendix of Notes and Tables refers to the Lectures on Ætiology.

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CONTENTS

LECTURE I

Diseases not unnatural—The natural history of disease—External causes of disease, mechanical, chemical, &c.—Effect of cold, direct and indirect—Contagion—Invasion of bacteria and animal parasites—Origin of disease in persistence of occasional physiological states—Habit and tolerance—Vulnerability and resistance—Origin of acute from chronic diseases—Congenital and hereditary diseases—Diathesis and predisposition—Degenerative diseases—Overwork and modern life.

Mr. President and Gentlemen,—In asking your attention to certain points in the ætiology of disease, I would begin by submitting that the existence of disease in general needs no further explanation than the existence of life. Disease is as natural, as physiological, as health. No living thing is immortal, and life is a constant struggle against surrounding forces, in which each individual organism succumbs after a while, by handing on its life to its successor, when it has secured the victory of the race.

> Sic rerum summa novatur
> Semper et inter se mortales mutua vivunt;
> Augescunt aliæ gentes, aliæ minuuntur,
> Inque brevi spatio mutantur saecla animantum,
> Et quasi cursores vitaï lampada tradunt.

Pathology is only a chapter of physiology, which for convenience we have separated from the rest. We call that which brings us discomfort "disease," and look upon disease as obtrusive, abnor-

mal, unkindly; but, if we knew it, the highest temperature of fever is as strictly in accordance with the laws of animal thermogenesis and thermolysis as the slight diurnal oscillations of health; and the excessive secretion of the kidneys in diabetes is as much a part of the system of nature as the moderate increase produced in a healthy man by a cold east wind or by copious draughts of water.

The processes of degeneration and decay which lead to death should be regarded as correlative with those of growth and development which follow birth. It is as natural for the old man to shrink in bulk and droop in stature, for his—no longer permanent—cartilages to ossify, his hearing to grow dull, and his memory weak, as for the child to grow, for his bones to set, and his mind to develop.

It has long been known that human beings are liable to be the hosts of parasitic beings less highly organised, but living, growing, and reproducing, like ourselves; and in recent times we have learnt that in addition to the diseases produced by the presence of animal parasites, another large group is due to the invasion of countless hordes of minute parasitic plants; but to regard filariæ and bacilli as merely "causes of disease" in man, is to take too narrow, too human a view.

Every kind of beast and bird and fish, and many of the lower classes, are known habitually to harbour parasites. In the majority of cases this is without injury to the host; indeed, if the parasites were in-

variably fatal, the victory of the individual would prove destructive to the race, since, if a host can live without a parasite, the parasite cannot live without his host. The fact is that if we call "disease" whatever is abnormal, then the "pathological" condition is to be *free* from parasites. The bare and inhospitable state in which the most civilised of mankind keep their bodies, is only maintained by ceaseless care and vigilance; as soon as these are intermitted, numberless evicted tenants resume their natural right of domicile. The immense majority of our fellow men, in the past and in the present, have been, and are, the contented hosts of a dependent fauna which finds as natural and unblamed a habitat on or in the human body, as birds in the air or fishes in the sea.

From the point of view of the tænia, the trichina, or the pediculus, man is in general part of its environment, and, in particular, its homestead and its barn; while sanitary, economical, and pathological questions from the parasitic point of view refer not to the host, but to the guest.*

Let us then, for this occasion, look upon diseases not as unnatural, lawless, heterogeneous, abnormal, revolutionary; but as parts of the vast order of Creation. They are not to be "explained" by any single all-mastering principle of physical evil. The various ancient systems of pathology which pre-

* See Dr. Payne's concluding remarks in his admirable paper on "Specific Diseases and Parasitism," in the 'St. Thomas's Hospital Reports' for 1892.

tended to discover a common origin of disease are all outworn, dead, or dying—as obsolete as the correlative systems of therapeutics which pretended to deal with disease as a whole by some universal antagonistic principle. Like is not cured by like, nor contraries by contraries; the true physician is not guided by fallacious general hypotheses, but by individual insight and experience; for as the greatest of philosophers taught—ἰατρεύει καθ' ἕκαστα.

Let us, then, investigate ætiology as the natural history of diseases, of the circumstances in which they arise, and the complicated conditions on which they depend; and disease itself as the necessary, inevitable result of the construction and working of the body—"the very pulse of the machine."

We, in our daily duties as physicians, cannot, I admit, take so philosophical a view of disease; what we cannot prevent, we remove, what we cannot remove we alleviate; if we are unable to reverse the capital sentence under which we all live, we strive to postpone its execution as long as possible. We are the sworn foes of intemperance and of vice, of darkness, and of dirt. Our weapons are air and light and exercise, "places and waters," diet and abstinence, heat and cold, electricity and drugs. Our allies are phagocytes and chemotaxis, and the inertia of an organism which tends "to go on." The combat is not without success, and when medicine has reached her limit, she can often direct the more potent hand of a younger brother,

and make way for the conquering steel, when *dignus tali vindice nodus*. This practical, therapeutical side of medicine is ever with us; common humanity and the peculiar interest that each separate case excites prevent any danger of our maintaining a philosophical tolerance of disease. What we are most apt to forget is its scientific side. The study of the causes of disease, or rather I would say of their *natural history*, *the circumstances in which they occur*, is theoretical rather than practical. Yet it tends ultimately to advance the art as well as the science of medicine. For successful treatment depends on diagnosis, and diagnosis rests upon pathology, and pathology in its widest sense includes not only the anatomy but the physiology of diseases—not only their "seats" but their "causes."

Moreover, if inquiry into the origin and conditions of disease helps treatment, it is indispensable for the still better art of prevention. That depends entirely upon the extent of our knowledge of ætiology; and I venture to believe that more human suffering has been relieved, and more lives have been saved, by the studies in natural history, which have taught us the origin of infectious fevers, of scurvy, of lead colic, of erysipelas and pyæmia, than by the best results of pharmacology.

We still keep in memory the names of Paracelsus, and Glauber, the Marchioness of Cinchon, Dr. Dover, Dr. Gregory, and Mr. Plummer, but who would compare these twinkling asteroids to the

splendour that surrounds the names of Edward Jenner, of Pasteur, and of Lister?

External Causes of Disease.

Let us first consider whether it is possible to classify and appreciate the several causes, external or internal, which alter or modify the ordinary working of the human machine, in such a manner as to cause pain or discomfort, to incapacitate any of its ordinary functions, or to lead to a cessation of the entire mechanism. The human body, like that of all the Metazoa, is an aggregate of more or less independent living particles. Not only in the ascent from the simplest to the more complex plants and animals, but also in the development of each individual, we start with the single nucleated mass of protoplasm. Each living cell, like the organism of which it forms an element, can feed, digest and assimilate, can breathe and excrete, can be irritated, and can move. In this microcosm

> "Nature's self untwisted lies,
> Into its first complexities;"

and, when thus unravelled, the problems of life which we vainly sought to solve, when we understood the mechanism of circulation and secretion and movement, are seen stripped of such secondary additions, reduced to their naked simplicity—and more insoluble than ever.

Now if the life of a single cell be the life of the whole organism in miniature, so will be its death and

the processes which lead thereto. What are the external agents which can injure or derange the ordinary functions of the cell? They may be thus stated: —(1) *Mechanical* injuries; (2) *Electrical* stimulus; (3) *Chemical* agents; (4) Extremes of *heat or cold.* All these, which in moderate degree act as stimuli, when more powerful become destructive. In addition, the living cell may be (*a*) *starved* for want of food, (*b*) *stifled* for want of oxygen, or (*c*) *poisoned* by its own excreta. If none of these accidents befall it, yet its life is limited, for, after giving birth to a new generation, the impulse which gave it being gradually weakens, until its inscrutable molecular construction and the functions which that construction make possible, come together to an end.

We see essentially the same disturbing forces in their action on the entire organism.

(1) Disease may be caused by mechanical *injury.* To this group belong what used to be distinguished as surgical diseases, and, above all, traumatic *inflammation*, the reaction of the injured tissues. We may also regard as traumatic the effects of concretions, such as gall-stones and urinary calculi; their formation is a separate event to be explained on chemical grounds, but the disease is not the foreign body thus formed, but the effect of its presence upon the living tissues with which it is in contact.

(2) The effects of *poisons* come next, but although they agree in their action being chemical, it is probable that this is their only agreement. We are, at

present, quite ignorant of the physiological action of hydrocyanic acid upon the ganglion cells of the bulb, or of phenacetin upon those of thermotaxic centres, and of strichnia on those of the reflex mechanism in the cord, as ignorant as when candidate for the doctorate in *Le Malade imaginaire*, thus explained the properties of opium:

Mihi a docto doctore
Demandatur causam et rationem quare
Opium facit dormire.
A quoi respondeo
Quia est in eo
Virtus dormitiva
Cujus est natura
Sensus assoupire.

(3) Nothing is positively known of the effect of *electrical* and *magnetic* changes in the air, the earth, or the body itself, in producing disease. May we not add that, if we except such definite effects as can be produced in the moist chamber (such as stimulation and electrotonus), the therapeutical effects of electricity, whether statical, galvanic, or faradaic, are equally uncertain.

(4) Our knowledge of diseases caused by chemical agents is much more extensive and exact. The effects of prolonged administration in minute doses of metals, such as mercury, lead, arsenic, silver, is well ascertained. The discovery of the true cause of Devonshire colic by Sir George Baker is a classical instance of ætiology by inductive reasoning.

The effects of organic poisons is more obscure, but in one case at least, that of alcohol, we have

made great advances, both in range and precision of knowledge, since the first cirrhotic liver was described to the Royal Society in 1685.*

Moreover, what was always a more or less probable conjecture has now been proved in certain cases to be a fact, namely, that the effects of the most numerous and destructive parasites are due to chemical poisons which they either excrete or manufacture from the normal proteids of the body. The important paper of MM. Roux and Yersin, which appeared in the 'Annales de l'Institut Pasteur for 1888,' proved this by direct experiment in the case of diphtheria. The ptomaines are no longer vague alkaloids, but are produced as definite crystalline combinations with platinum, and Dr. Sidney Martin's thorough and judicious researches on the poisonous albumoses are fresh in our memory.

(5) Of *heat or cold* the direct effects, as we see them on the external surface, are readily understood. One or two considerations, however, perhaps deserve mention here; one is the remarkable difference between the effects which the same degree of heat or cold will produce upon different skins. We have always the "irritabile" to consider as well as the "irritans." Another is the remarkable effect of habit upon living tissues, as well as organisms, upon epithelium and glands as well as on the ner-

* By John Browne (author of 'Myographia Nova,' 1684). His figure in the 'Philosophical Transactions' is reproduced by Dr. Payne in his learned introduction to the discussion on alcohol at the Pathological Society (vol. xl, p. 314).

vous system: and habit tells in two ways, by inuring tissues to bear without reaction what at first was a powerful irritant, and again in continuing the effect of an irritant which has passed away. So that chronic eczema will follow brief exposure to the sun, and chronic bronchitis may result from a single chill.

Catching cold.—A much more abstruse effect of heat and cold is that which is indirect, and affects not the skin or superficial mucous membranes, but organs which are too deep to have their temperature altered by a direct external cause. How far is the popular, and we may add medical, belief justified, that common catarrhal laryngitis and bronchitis, pleurisy and pneumonia, inflammations of the stomach and bowels, the kidneys and liver, and even of the spinal cord, are due to external cold?

If we try to define this process of taking a chill, we might, in the first place, suppose that cold acts directly upon the nearest underlying important organ. Some apparent cases in support of this view do not really apply; for instance, a cold wind even without the help of dust may produce injection of the conjunctivæ, but this is an immediate application of cold to the organ, just as cold water, or cold air, may act upon the exposed skin and produce a slight dermatitis. Nor is the undoubted production of facial paralysis a case in point, for there some impression is directly made upon the periphery, perhaps the muscular end-plates of the portio dura; degeneration follows, but there is no evidence of

neuritis. Again, sore throat or bronchitis following exposure to a dry, cold air are only fresh examples of cold acting as a direct local irritant. Both affections are readily produced in many persons by talking out of doors, or by the habit of breathing through the mouth, a habit which often forms part of an unlucky circle, since it is caused or kept up by chronic tonsillitis (including the pharyngeal tonsil) obstructing the entrance to the posterior nares, or by swelling of the Schneiderian membrane from coryza obstructing the anterior entrance.

But do we find that bronchitis or pneumonia or pleurisy follow the application of cold to the chest, and that enteritis or peritonitis is produced by cold on the abdomen, or nephritis by cold upon the loins? Such is the popular, perhaps we may say the common medical belief; but is there adequate evidence of its truth? The trunk is so well covered in cold and temperate climates, that it has scarcely any chance of being chilled except by the clothes being wet through. Granting that this is a cause of acute illness, is it not as often followed by rheumatic synovitis of the extremities, as by pleurisy or pneumonia? Is it not much oftener followed by coryza, sore throat, and cough? Is it ever followed by peritonitis? There is a belief, not without foundation, that acute myelitis is often occasioned by working in water; but in all the cases of the kind I have ever seen, the wet and cold has affected the lower extremities rather than the spine.

No one can suppose that cold applied to the surface could possibly produce by direct conduction a local fall of temperature in organs so deeply placed as the spinal cord and the kidneys; but it is imagined that some reflex motor or trophic influence is excited, an hypothesis analogous to that which is used to explain the employment of blisters and other counter-irritants to relieve deep-seated inflammation. The evidence for such an action appears to be inadequate; the vascular communication between the parietal and visceral pleura, between the lumbar and renal arteries, is but small and irregular. We have no evidence of the existence of a nervous relation of the kind supposed, and there are, I believe, no adequate clinical facts to support the hypothesis; on the contrary, many facts are against it; for instance, the impunity with which we place ice-bags upon the head, the thorax, and even, in cases of intestinal hæmorrhage, upon the abdomen; this treatment never produces inflammation of the lungs, the brain, or the bowels. Then it is held by many good physicians that ice applied to the chest can not only check pulmonary hæmorrhage—which I confess I doubt—but also pulmonary inflammation, which I venture to disbelieve. It will scarcely, at all events, be held by the same physician that pneumonia is both caused and cured by cold applied to the chest.

We seem thus to be driven to the belief that cold applied to the surface of the body has no local result

beyond the effect upon the skin; that a person may be exposed to severe and long-continued cold, causing acute suffering and even local lesions, like frost-bite, without any internal inflammation. On the other hand, we often seem to "catch heat," rather than cold, in crowded rooms and over-warmed houses. This mischief seems to be, not the cold, but the chill, a sudden impression which we sometimes feel when exposed to a draught on going out of a hot room in winter, or on sitting, after profuse perspiration, in summer,—an impression which we sometimes feel and sometimes do not. This chill is accompanied by a rigor, often represented by nothing but a slight shiver, or by a convulsion in the shape of a sneeze; we feel at once that the mischief has been done, and a catarrh or some other inflammatory disorder follows.

Most of us have experienced in our own persons the sudden chill which produces sneezing and coryza, or the repeated effects of a cold east wind upon the face or hand which have the same effect; when wet through we must have felt the throat becoming sore and the voice hoarse, and in many cases we have also found that the remedy for "a *cold*" is to keep *warm*, that a change of clothes, a hot bath, or a few hours in bed will cut short the incipient attack as no specific fever can be cut short.

The distribution of catarrhs, bronchitis, and pleurisy, both as to seasons and to climate, is further evidence to the same effect.

Nevertheless the result is far from invariable, and is not determined by the locality or the severity of the chill. In most persons in this country and in temperate Europe and America, a coryza with sore throat, followed by a slight catarrhal bronchitis, is by far the most common result; but in many persons, especially in the summer months, the same kind of exposure of the face or head will produce diarrhœa and other symptoms of catarrhal colitis, local pain, irregular peristalsis and discharge of mucus, with furred tongue, loss of appetite, and slight rise of temperature. In India, China, and the Southern States of America a precisely similar chill produces an attack of ague. These three effects are well substantiated by an enormous mass of evidence—evidence which probably most in this theatre could supply.

Does such a "chill" sometimes cause rheumatic fever, pneumonia, acute Bright's disease, pleurisy, or acute myelitis? The evidence, though far less convincing than in the three cases before mentioned, seems too strong to be set aside; but we must admit that each of these effects is comparatively rare, that each of the acute inflammations enumerated frequently occur *without* exposure to cold or wet, and without any premonitory chill, and that, granting the general ætiology in question, we cannot pretend to say why the same cause should in one case produce rheumatic synovitis, in another pneumonia, and in

a third acute nephritis. Moreover, we must not forget another analogue of these diseases, quite different from that of a catarrh. In many particulars pneumonia resembles a specific fever rather than a local inflammation, and though rheumatic synovitis is undoubtedly a true inflammation, yet it is but a part of rheumatic fever. May we therefore compare the chill which ushers in an attack of lobar pneumonia, or of acute rheumatism, to the chilliness, the rigors, or the convulsions, which are often the first symptoms of scarlatina, smallpox, or typhus.*

Contagion.—Of *contagion* as an external cause of disease I must say but little, partly because the subject is so vast, partly because it has already been so ably treated, and so recently. I will only venture the following remarks upon this part of my subject.

* We cannot, I think, accept the bold negative proposed by Dr. Ransom, of Nottingham, in denying that exposure to cold is the cause even of a cold in the head. Even if we put aside the universal popular belief as to an extremely common malady, to which all civilised languages bear witness, if we put aside the prevalent medical opinion, which is unbiassed by any of the theories that have dominated our schools, we cannot explain away the direct experiments which are constantly being made in this particular. A cold in the head, though often and in many people accompanied by true fever, and running a tolerably definite course, is in others, or in the same patient at another time, a mere local inflammation. It is notoriously more common in winter than in summer; it can usually be traced to a definite exposure to cold, and does not come on without such exposure; it can be checked or aborted, if when the chill is felt, warmth is at once restored; and, even when established, its symptoms can be entirely removed for a time by a hot bath, or can be quickly, safely, and pleasantly cured if the patient will submit to confinement to bed for a day or two.

When the bacterial explanation of contagia is criticised—and it is well that so dominant a theory should be criticised,—and when we are told, truly enough, that in very few cases have diseases been definitely and rigorously proved to depend upon the presence of a specific microphyte, and that in some of the best known and most undoubted specific fevers none has yet been discovered, in typhus to wit, and smallpox, and measles—we must remember two important considerations in reply.

First, it is abundantly clear that these diseases do not arise *de novo*, that they are strictly contagious and breed true; it is equally certain that they are not conveyed by gaseous emanations or liquid secretions, or by immaterial forces. The fomites of plague and of scarlet fever convey the material contagion, that of variola can be collected, seen, and handled, like so much arsenic or prussic acid; it is particulate, it multiplies indefinitely, it is preserved or it is destroyed by the agencies which affect only living matters. It is therefore a *contagium vivum*, and that being proved, it is perhaps allowable to assume for the present that it is more likely to turn out one of the schizomycetes than a fungus, a microzoon or a living organism which cannot be referred to any group at present known.

Secondly, although the complete demonstration of the bacterial nature of a disease has only been yet successful in four or five cases in man, yet the diseases as clearly demonstrated to be bacterial in the

lower animals are more numerous, and under the conditions of observation and experiment we may perhaps rather wonder that the complete proof in the case of human diseases is present at all, than that the process of arriving at it is slow, and the instances as yet few.

Nevertheless the presence of microphytes, even when constant and pathogenic, does not determine the whole character of a disease. On the one hand we have such affections as tubercle, where the state of the soil is of greater consequence than the presence of the seed; we have the case of leprosy, where the bacilli, though constantly present, are in such small numbers that we cannot assign them more than an initial importance in the result; we have cases of undoubtedly bacterial disorders, which are neither contagious nor general, as lupus; and lastly, there are bacterial and general diseases in which the microbe appears never to be distributed over the body, but to remain confined to the seat of inoculation, and thence to affect the distant organs by its chemical products, as diphtheria and tetanus. Theoretically we might suppose that microphytes produce a pathological effect in any of the following ways:

(*a*) By local mechanical irritation, in the same way as the acarus or pediculus; such irritation could only produce a circumscribed and common reaction, and could not account for specific infection. (*b*) Secondly, by mere bulk, their rapid

multiplication obstructing capillaries or lymphatic paths, after the manner of an embolus. This, however, could only produce hæmorrhage, œdema, or other local disturbance, like the effects of the *Filaria sanguinis*. (*c*) Since these minute plants grow and multiply after inoculation, they must consume an enormous quantity of material from the blood and lymph; hence it is conceivable that they might starve the host, like intestinal worms when in unusual numbers. (*d*) Again, they must consume oxygen, though probably the aggregate amount is only small, and they must excrete carbon dioxide, though this also would be but a fraction of that normally present in the blood. (*e*) Lastly, they must excrete other materials, analogous to the urine of higher organisms, in so far that they are nitrogenous in composition and more or less toxic in their effects on living tissues. That this is the way in which the infective or toxæmic action of microphytes is accomplished seems clear from the specific effects peculiar to each species, and has now been demonstrated in the case of diphtheria by inoculation with the products of a specific microbe when the organism itself has been carefully excluded.*

* We may notice in passing, that the production of artificial diphtheria by the inoculation of toxines in solution, derived but separated from the specific bacillus, contrasts with the results obtained by Chauveau and by Sanderson in the case of vaccine lymph which is deprived of its specific power by filtration through porcelain.

Apart from the mode of *action* of bacteria, another point in the ætiology of infective diseases is the *method of their transference*. Probably the virus, though discharged by many channels, has a much freer, more constant, and practically important outlet by a few or by one; and although capable of entering by many portals, finds it so much easier by one, that if we can preoccupy this, we may protect the whole organism from the invasion. Such channels of predilection would seem to be—for Typhus, the expired air for leaving, and the inspired air for entering the organism; for Enterica, the fæces for exit, and drinking-water contaminated by sewage for entrance; for Scarlatina, the epidermis and the urine for discharge, and the breath for ingress; for Measles, the mucus of the inflamed air-passages; for Smallpox, the serum and crusts of the eruption; for Mumps, the saliva of the affected glands; for Cholera, the rice-water discharges; for Tubercle, the dried sputum.

In some cases we can, more or less certainly, point to several places of invasion; thus the dried pus of smallpox is usually conveyed by the inspired air to the stomata of the air-vesicles, and thence by the lymph and blood throughout the body; but we know from the effects of inoculation that its contagion finds a ready entrance through the nasal mucous membrane. There is some evidence in favour of occasional invasion of enteric fever through the air and through solid food, as well as through

water, and also of the conveyance of tubercular infection by milk and by meat from tuberculous cows.

There is still difference of opinion as to the precise botanical position of the microphyte known as Actinomyces; but whether it belongs to the schizomycetes or to a higher group of fungi, its effects are more local and irritative, and its secondary growth follows more restricted anatomical paths than we observe in the case of ordinary infective bacteria. There is now good reason to believe that actinomycosis is not conveyed by contact from diseased animals to man, nor yet by eating the flesh of an animal affected by the disease; it is probably, in all cases, directly derived both by men and animals from the barley or other cereals infested by the parasite.

A striking proof of the need for caution in accepting conclusions before they are proved, and the fallacy to which reasoning by analogy is liable, is afforded by the case of the ætiology of Ague. A few years ago the presence of a specific microphyte was assumed, and its discovery anticipated with as great certainty as we now feel in the case of typhus or measles. But the researches of several eminent pathologists in this direction were either fruitless or led to unsatisfactory results. At present the evidence is all but conclusive that malarial diseases are constantly associated with the presence in the blood-discs of a contagium vivum, or, if we please

to call it so, a microbe, but not a bacillus or any kind of microphyte.

The presence of another distinct kind of protozoa, the organisms known as psorosperms and coccidia, has long been recognised in the liver of rabbits, and other cases in which its effects are purely local; but recently, first in France, and afterwards in this country, similar structures have been recognised in the epithelial cells of molluscum contagiosum and of certain varieties of epithelial cancer. Their ætiological significance is at present doubtful, but if it be established, we shall have a new form of parasitic life, producing disease in an entirely different way from bacteria on the one hand, and from the hitherto recognised epizoa and entozoa on the other.

To resume: let us come back to the primordial cell as a representative of the organism of which it is the origin. It is, we have seen, exposed first to what may be called physiological deprivation, *i. e.* it may fail to obtain sufficient food or moisture or oxygen for its needs, and perish, as multitudes of plants and animals perish, without coming to maturity at all. Next it is exposed to what we may call accidental and external disturbance, excessive cold or heat, electrical or chemical excitation of too violent a kind, and mechanical injuries. In the human subject, while the extreme degree of these influences has a local destructive effect, which is readily identified, slighter but repeated variations produce indirect results which are more difficult to

follow; such are the effect of cold in producing internal inflammation, the effect of heat in sunstroke, the effect of chemical poisons whether irritant or narcotic after absorption, and the effect of slight but repeated mechanical injuries in giving rise to chronic structural lesions. The third group of disturbing causes which pervert the physiological action of the cell is that of parasites, other living organisms which invade and destroy it.

Physiological Origin of Diseases

We now come to an important group of *internal* causes of disease, namely, disturbances of one or more of the bodily functions, which are at first merely functional and do not pass the limits of normal physiology. For example, it is natural for the cardiac contractions to become more frequent under mental emotion or bodily exertion; in a healthy and vigorous man who runs a short distance without previous training, not only does the heart beat much more rapidly than when at rest, but its contractions become violent and irregular, and a loud blowing murmur may show temporary tricuspid incompetence. If the exertion is repeated moderately, systematically, and in a young and healthy subject, the effects described cease, and will only reappear if he falls out of training, or if he exceeds the limit of exertion for which he is competent; or if he runs under unfavourable circumstances, as, for example, with a distended stomach; or if he attempts to

repeat the feats of his youth, when increase of adipose tissue, stiffness and inelasticity of organs, and perhaps muscular degeneration have come with advancing years. But when violent mental emotion or excessive bodily exertion occurs too often, what was a temporary physiological variation becomes a pathological habit; the heart is readily excited to palpitation, it does so on less and less provocation, and at last without any assignable provocation at all; what was an occasional has become an habitual deviation from the normal. Moreover, if such functional disorders continue, they more or less surely lead to structural changes—the irritable heart becomes hypertrophied; degeneration of its tissues follows; the overstrained aorta dilates; the valves become incompetent, and the whole vascular apparatus falls into irremediable ruin.

Again, a cold breath of air upon the face provokes a sneeze, or the presence of sulphurous acid gas in the inhaled air excites a cough, and the reflex mechanism called into action in each case is rightly claimed by the physiologist as part of the wonderful working by which the body is protected and preserved. But let the same irritants be too long or too often experienced, and, in addition to the reflex nervous mechanism, there ensue nervous changes—vaso-motor, secretory, and trophic,—until the draught produces a catarrh, and the cough becomes bronchitis.

Not less frequent is the same progress of events

in the digestive system; the stomach is supplied with food in too large quantities or at too short intervals, or imperfectly prepared by mastication and insalivation, or the vascular and secretory activity of the gastric mucous membrane is disturbed or completely inhibited by ill-timed cerebral excitement: the result is a physiological check to gastric digestion, and the undigested food becomes a foreign body and acts as a local irritant; the process, if long continued, leads to chronic gastritis, or perhaps to dilatation of the stomach, or the peptic glands strike work and atonic dyspepsia results.

Again, inattention to natural calls may lead to nothing worse than irregular action of the bowels; but when the colon has become habituated to fæcal distension persisting for many days, not only does its reflex peristalsis become dull and sluggish, but mechanical dilatation ensues, and we may at last have a condition of true intestinal obstruction developed from functional disturbance.

Similar illustrations from the menstrual function in women are only too common. I will only add one more. When certain articles of food, particularly spices and alcohol, are taken in more than a very moderate amount, the result is temporary vaso-motor paresis of the face; dishes compounded with curry or chillies will produce this effect in almost every case when admitted to a virgin stomach. If the degree of stimulation is moderate, and the self-adjusting power of the organism good, the nervous system becomes

habituated to the stimulus, and the effect no longer follows; but if either of these conditions be absent, the flushing after meals becomes habitual, until it is excited by wholesome food or by diluted alcohol. We then have a frequently repeated local erythema of the face which we may compare physiologically with the erythematous and urticarial eruptions produced by drugs and by special articles of diet. Recurring after every meal, this vascular dilatation at last no longer disappears in the interval, the skin and subcutaneous tissues hypertrophy, the sebaceous glands are in the condition of the submaxillary during stimulation of the sympathetic nerve, and the result is a permanent structural disease, gutta rosea—erroneously called acne rosacea—a condition always, I believe, associated with flushing after meals, though it is often accompanied in women about the climacteric period with ovarian disorder. Although the gastric irritant is, in most cases, some form of alcohol, yet the disease is produced not by alcohol directly, but by the gastric irritation engendered thereby. Without the latter there will be no trace of gutta rosea in a patient suffering from delirium tremens or cirrhosis of the liver, while some of the worst cases of this unsightly malady occur in the most abstemious of women.

With respect to this large and important group of diseases which have their origin in physiological disturbance gradually becoming more frequent, and which end in permanent structural disease, we have

always to bear in mind that, to use a homely proverb, it "takes two to make a quarrel." So it takes two to produce a disease—the blow and the reaction, the irritant and the patient. When a man complains that wholesome food disagrees with his stomach, we might retort that his stomach disagrees with its food. What produces a slight and transient disturbance, or no disturbance at all, in one person will seriously and perhaps permanently derange another. Much depends upon habit and practice, particularly on voluntary habit and regulated practice. The muscular exertion which would produce utter exhaustion, with feeble pulse, cold sweat, complete anorexia, and rejection of food if swallowed, in the case of a young and healthy man who has never tried his powers, is borne with no effect but the pleasant sense of fatigue and increased digestive powers by an athlete in full training. The amount of liquor which in the case of an unpractised drinker would simulate mania or paralysis or coma, will scarcely dull the senses of a practised toper. The few whiffs of tobacco which produce vomiting and something like collapse in a schoolboy, have no effect whatever when, by reason of use, the senses are exercised thereby. The same consideration is forced upon us by the action of irritants upon the conjunctiva and the skin, where we can observe all gradations from purely traumatic inflammation limited in duration by the irritant, to that which, though thus set up, continues long after

it has ceased to act, and finally to inflammation produced by no external cause, and therefore, as we say, idiopathic. If this is true of inflammations in general, it equally applies to the reaction of the organism to other mechanical, thermal, or chemical irritants, and above all to its behaviour under the influence of animal poisons. Some of these appear to be so violent and efficient that all they need is human tissues to act upon; the contagion once brought into effectual contact with the organism produces an invariable and inevitable sequence of effects. Such, or nearly such, may be said of syphilis, of vaccinia, and of glanders, to take three striking examples; but even here the effect is not so entirely dependent on the contagion and independent of the recipient as would at first seem. Witness the different mortality of typhus in the young and the aged. It seems at first sight as though sufficiently violent poisons would produce approximately the same effects upon any living tissue. Even this, however, is not proved; we put down such and such as the lethal dose of arsenic or laudanum, but we know that opium-eaters in Lincolnshire and arsenic-eaters in Styria take much larger quantities with impunity. Morphia has no effect upon pigeons, nor belladonna upon rabbits; and both arsenic and belladonna can be given to children in far larger doses than the proportion of their body-weight to that of adults would dictate. Measles, smallpox, and influenza do not attack every

individual even when they invade an unprotected population; some animals, though in other respects closely allied, are incapable of receiving certain contagia—syphilis for example, hydrophobia, anthrax, and relapsing fever. The protective influence of most, though not of all, the specific fevers against their own recurrence is a familiar fact, and belongs to the same order as the artificial protection from small-pox afforded by its attenuated virus—vaccinia—and the artificially produced refractiveness of animals to certain diseases which we owe to the genius of Pasteur. All of these are examples of an efficient cause of disease failing for want of a suitable object to play upon. An old man is less obnoxious to enteric fever than a child; a fasting man is particularly liable to catch typhus; a population in which measles has been endemic for centuries has only impaired susceptibility to its contagion, so that we only learn how terrible this disease can be, when it is allowed free play on virgin soil as in the Fiji Islands.*

There must be conditions at present unknown which protect a large number of children from scarlet fever, though exposed to infection; for it does not run through schools with the same freedom as mumps or measles. Nay, in some cases the susceptibility of the organism varies so widely that it be-

* When I was a student at Guy's Hospital in 1863, the crew of a Peruvian corvette lying in the Thames were attacked by measles, and four or five men who were admitted to Mark Ward all died with pneumonia.

comes a more important element in the production of disease than the existence of contagion itself.

Acute Diseases often begin chronically

This important point in the ætiology of diseases I learnt from Dr. Wilks; and although I have not met with the observation elsewhere, it will, I feel sure, be backed by the experience of those whom I address.

It is a commonplace of medical writers that acute diseases, instead of terminating by resolution, often continue in a mitigated but protracted form, and hence the malady which is at first both sharp and short (ὄξυς) becomes less severe and more protracted (χρόνικος); hence the adage, *Si dolor gravis, brevis; si longus, levis.*

But though this is often true of rheumatic fever when untreated, of successive attacks of gout, of eczema, of tubal nephritis, and of malarial fever, yet the opposite order is not less important to observe, viz. that acute diseases are often developed out of chronic. The sudden seizure of a man in all the vigour of youth and health, by which the strength and ease of the morning is changed in a few hours into prostration, severe suffering, high fever, and a perverted state of every function of mind and body, is so striking and dramatic an event that it naturally arrested attention. Such cases have been described by the best medical writers, who naturally choose the most striking

and picturesque aspects of disease. But probably the only group of diseases in which such a mode of onset is at all common are those which are contagious and depend upon the invasion of a healthy organism by a countless swarm of foreign organisms attacking it from without. They are so numerous and their poison is so powerful that they at once overcome the utmost vigour of youth and health, just as the strongest man is immediately subdued by stifling gases, or by the venom of a serpent.

In most other cases of disease, however, the apparently acute outbreak is really the result of a less obvious, but no less important antecedent failure of health; even in the case of infective diseases, experimental pathology teaches us that while on the one hand an organism may be rendered refractory to the invasion of the virus, on the other hand it may be artificially prepared (as by the injection of the cultivation fluid of blue pus) so that the subsequent invasion of a very attenuated virus shall produce the most severe effects. It is a matter of common experience that enteric fever often begins with days, or even weeks, of diarrhœa; hence arises, at least in part, the difficulty of fixing the precise date when the specific fever began. May it not be that the local condition of the intestine renders it more obnoxious to the action of the microphyte when it invades? In the same way genuine diphtheria is often preceded by non-specific

and subacute angina; and may not here again the inflamed mucous crypts and lymph follicles and desquamating epithelium prepare a favourable nidus for the reception of the specific bacillus? In the case of phthisis there is even reason to believe that the bacilli of tubercle find no reception in a perfectly healthy organism, and only make a successful inroad when local pulmonary catarrh or general feebleness of health have prepared the way for their reception.

But if this be to some extent true of contagious diseases, it becomes the rule, and no longer the exception, when we study the onset of many non-specific, but acute, inflammatory disorders. Abscess of the brain is acute, severe, and rapidly fatal when once developed, but it never attacks previously healthy patients; it is always preceded by injury, by caries, or by primary suppuration. How often do we find acute lobar pneumonia, a disease in many ways allied to a specific exanthem, not occurring in its classical form as a primary attack in a previously healthy man, but as a secondary complication of cardiac disease, of enterica, of rheumatism, or of chronic Bright's disease, often in patients who are bedridden and have never been exposed to cold and wet? Acute suffocative bronchitis is very rare in the young and healthy; the vast majority of cases occur in old and decrepit persons who have long been subject to winter cough, emphysema, and consequent dilatation of the right side of the heart.

The cases of phthisis which are most inflammatory, cases of "galloping consumption," are almost always less rapid and acute than they seem. Careful inquiry brings out in most cases the fact of failure of strength, loss of flesh, and sometimes of definite local symptoms in the chest or throat, which have long preceded the apparently sudden outbreak of the disease. Phthisis *ab hæmoptoë* is not infrequent as a clinical event, although pathologically hæmorrhage is probably never the origin of phthisis.* But when a patient tells you that while in the enjoyment of vigorous health he burst a blood-vessel under the stress of some sudden effort, how often do we find that scarcely noticed preceding symptoms of pulmonary disease have been present, while the physical signs point to changes no longer recent.† How frequently after death from what seemed to be an unusually rapid case of phthisis does the post-mortem room reveal long antecedent and partially obsolete lesions.

* Herodotus relates the following story in point:—There were three generals of cavalry in the army of Xerxes, and one of them, Pharnuches by name, was left behind sick at Sardis when the troops moved on towards Greece. His sickness was the result of an accident. While he was riding at speed a dog chanced to run under his horse's legs. The horse was frightened, reared, and threw his rider. He brought up blood, and the injury ended by turning to a consumption (lib. vii, cap. 88).

† Dr. Richard Morton in his 'Phthisologia' (lib. iii, cap. 5) remarks:—"Hoc tamen perpetuo fere observare licet, quoties scilicet hæmoptoe præcedit, phthisin pulmonarem subsequi solere." He also in the following chapter, "De phthisi a calculis in pulmonibus generatis," describes cases of expectoration of earthy concretions which we now know to be the result and not the cause of consumption.

The slow and insidious onset of the interstitial form of Bright's disease leading to the contracted kidney is well recognised. Even in cases of tubal nephritis (apart from scarlatina) are not the more acute symptoms in many and perhaps in most instances preceded by others less obvious? In the cases of that deeply interesting and peculiar disease, acute yellow atrophy of the liver, which have come under my own notice, I have been struck with the frequency of this Icterus gravis developing not suddenly, but out of previous apparently benign and functional jaundice. Even mechanical lesions, like volvulus and other terribly acute and severe forms of intestinal obstruction, are often unexpectedly shown at the autopsy (without which records of such cases are all but useless) to be only the final result of slow and gradual changes which prepared for the final catastrophe.

Without denying that external infection may one day be shown to have a share in the production of cancer, must we not admit that new growths are in many cases local deviations from the natural histological life of a tissue? This view appears to be confirmed by their prevalence at certain periods of rapid development, as in the sarcomata of the bones and joints in children, but particularly at the periods of involution, disuse, and degeneration. If this be true, we shall in many cases find that the wide, striking, and at first sight almost incredible perversion from the normal structure of the part is not so

of unprotected rivalry. In the human subject, congenital diseases form a comparatively small but extremely interesting ætiological group.

We must distinguish them into three diverse kinds—first, what are strictly malformations, deficiencies, redundancies, or abnormalities due entirely to some deviation in the development of the embryo; familiar examples are harelip, spina bifida, hypospadias, atresia ani, and arrested or abnormal development of the heart and great vessels. The last mentioned is of practical importance to the physician; it was dealt with by the late Fellow of the College, Dr. Peacock, with characteristic accuracy, and if he sometimes included under this head a larger proportion of cases, as for example those of tricuspid stenosis, than we should now admit, the reality of such errors of development remains.

When we have traced the origin of diseases to such congenital malformations we have reached the term of present, and perhaps of future knowledge; what determines such defects, excesses, or varieties, is now, and is likely to remain, a mystery. They are for the most part independent of any accidents of utero-gestation, since malformations are common amongst vertebrate and invertebrate animals, and also in the vegetable kingdom. Many, perhaps most, of such malformations may be regarded as extreme cases of "sports"—variations from the ancestral form, of which the laws are unknown and the existence unexplained by the theory of natural

selection which itself depends on their occurrence. I would only deplore in passing how slowly superstition gives place to reason, the lamentable proof of which is afforded by the belief that nævi, cleft palate, and deficient limbs are due to what are called maternal impressions—a survival pardonable and even interesting when we meet with it among ignorant villagers, but without excuse when it occurs in a learned profession.*

The second natural class of congenital diseases are those which, though present at birth, are the result of intra-uterine disease. Lesions of the right side of the heart from fœtal endocarditis (some would say rheumatic endocarditis), deposits of lithic acid in the fœtal kidney, intra-uterine ague and variola, are more or less authentic examples; but probably the most frequent and certainly the most important is congenital syphilis—a disease directly transmitted by infection from the parents to the offspring.

The third etiological group of congenital diseases are those which are derived by inheritance from the parents, but only develop long—sometimes many years—after birth.

To go back to our illustration of the entire organism by a single living cell—if we could distinguish the various accidents to which it is liable, and appreciate the importance of each, we should have only dealt with the irritants, the impressions from without. There would remain to consider

* See Appendix.

the protoplasm itself which was acted upon; its growth, its functions, and its duration would be determined by those of the cell from which it sprang. In like manner the ovum brings with it the properties of its parents.

The subject of hereditary disease is too wide and too important for treatment as a bye-work. Let us hope that it may be adequately discussed as the principal subject of some abler Lumleian lecturer. One or two remarks only will I venture to make.

Congenitally transmitted diseases, of which syphilis is the most important, do not come under this head.

Undoubtedly heritable maladies, as diabetes and cancer, are nevertheless in the majority of cases not inherited.

If, as some naturalists hold, acquired properties are not transmitted to the offspring, it is very difficult for us to explain the undoubted hereditary transmission of gout, hæmophilia, and phthisis.

As sources of fallacy in estimating the influence of heredity, we must bear in mind the probability of a son living in the same place, working at the same trade, and adopting the same habits as his father.

Again, all very common diseases will appear to be hereditary by mere coincidence. Hence statistical statements as to the frequency of rheumatic fever, cancer, or phthisis in the children of parents subject to these diseases, ought to be checked by control figures showing its frequency in others.

Lastly, we must not attempt to deal with obscure and ill-defined diseases; nothing valuable can come of statistics with respect to "rheumatism," "tonsillitis," or "epilepsy" without further definition. Yet we see jumbled together, in the attempt to establish an authentic inheritance, diseases of such different pathology as lumbago, rheumatic fever, gonorrhœal synovitis, and gout. We are told that a patient's eczema is hereditary because his father suffered from urticaria, a brother from "psoriasis" (possibly syphilitic), and his maternal grandfather from scurvy. Or a "neurotic" inheritance is established on the strength of convulsions and paralysis having been remarkably common in the family, when we find on inquiry that the cases consist of infantile paralysis, hysteria, traumatic epilepsy, hemiplegia from embolism, and paraplegia from tuberculous vertebræ.

In true hereditary cases there is transmission, not of the disease itself as in the case of syphilis, but of some structural or functional histological or chemical peculiarity which renders more likely than usual the development of such diseases as gout, insanity, or hæmophilia. A child born with a syphilitic rash and snuffles is not more likely than another to acquire syphilis if exposed to its contagion,—it has the disease already; but a child born of decidedly gouty parentage differs from another in the ease with which the same causes which produce the disease in general will be efficient in his case.

Hereditary tubercle, again, is only as a rare exception due to congenital transference of the virus, such that the infant enters the world with the bacilli in his tissues. In the vast majority of cases it means liability of the smaller bronchial tubes to catarrh, a vulnerability of the joints, and an excessive hospitality for the tuberculous microbe, or deficient capacity for dealing with it on the part of the phagocytes.

Diathesis and Dyscrasia.

It has long been the custom to explain the occurrence of certain diseases by referring them to a previous existing disposition, or (to use the Greek form of the word) *diathesis* of the patient; but it has always appeared to me that the assumption of such a cause is unscientific, and prejudicial to the advance of ætiology.

To say that a man who has had rheumatic fever is disposed to its recurrence is only to say that rheumatism is a disease which usually occurs more than once in the same person. We gain nothing by referring the occurrence of the rheumatism to a preceding rheumatic diathesis. If a man's father has had phthisis or gout he is no doubt predisposed to these diseases; but this again is only saying that phthisis and gout are often hereditary. The disposition or tendency to these or other diseases is either carried out or it is not. If it is never carried out, its existence cannot be proved and is of no practical

importance; if it is carried out, the right way to put the fact is to say that the disease has already occurred, and the supposed predisposition to it is only a roundabout way of repeating that assertion. If we find albuminuria and other signs of Bright's disease, we may, if we please, say the patient is predisposed to apoplexy. If a blue line is on the gums we may say that he is predisposed to paralysis of the extensors of the forearm; if he is a dram-drinker that he has a cirrhotic diathesis. But none of these proclivities would be counted diatheses in the conventional meaning of the word, and I venture altogether to deny, apart from actual morbid events, either present or past, from hereditary taint, or from such probabilities as depend on age or sex or occupation, that it is ever possible to predict the future advent of disease.

Along with this lingering remnant of theoretical and metaphysical medicine we ought surely to discard from ætiology such terms as "dyscrasia" and "temperament,"—phrases which belong to the exploded humoral system of pathology. We know that there is no such thing as the black bile, and that phlegm is not secreted by the hypophysis cerebri; how then can we talk of the due temperament of the four humours, or of their ill temperament, bad mixture, or dyscrasia? The division of men by their imaginary temperaments is arbitrary and without any basis, scientific or practical. It may be compared to the false sciences of phrenology and chiromancy.

Before leaving this part of my subject I would venture to urge a definite abandonment of the term "scrofula" in medicine. Its original application, as we all know, was to the swollen glands of the neck, and it included very different pathological conditions, since it was used at the time when the cervical lymph-glands, the secreting submaxillary glands, and the "ductless gland" which we now call the thyroid, were all supposed to be much of the same nature. In modern German usage the word struma is not, as in England, a euphemism for scrofula, but means what we call bronchocele. The traditional description of two types of scrofulous children, if it has any meaning at all, seems to apply to tuberculosis in the one case and to congenital syphilis in the other. To have separated the former condition and shown how it could be recognised, appears to me one of the principal achievements of English pathology in modern times; it is one of the many debts we owe to Mr. Hutchinson. We now know that what used to be called scrofulous diseases of joints and bones are examples of local tuberculosis. The adjective "scrofulous," when it has any definite meaning, means tuberculous, and "tuberculous" means "caused by infection with Koch's bacillus."

In the same way other adjectives should, I submit, only be employed in their strict and definite meaning. A rheumatic affection ought to mean an affection produced by, following, or belonging to rheumatic

fever. A gouty condition means, or should mean, the deposit of lithate of soda in an insoluble form in the organ or tissue in question. To use terms otherwise is to abuse our patients' credulity or our own, and the present arbitrary use of the epithet "gouty" will hereafter be regarded as we regard the use of "strumous" in the middle of the present century, and of "scorbutic" in the eighteenth.

Lastly, if the existence of these ætiological conditions is often arbitrary, what shall we say to the supposed blending of diatheses? Just as it was found necessary to add to the four classical temperaments and to admit "lymphatic" and "nervous" and "nervo-bilious," so it has been supposed that a hybrid disease like rheumatic gout can exist, and that scrofula and syphilis, herpetism and arthritism, may mix with each other and modify the effects of a wound or the symptoms of a specific fever. All such theories appear to me to be baseless and mischievous speculation, preventing serious inquiry into ætiology by assumptions of knowledge, and stopping investigation by premature explanation. So far as such vague and fanciful doctrines can be brought to the test they fail. On the one hand, our present knowledge of the true ætiology of syphilis, malaria, scabies, scurvy, gout, and tubercle, forbids our admitting the possibility of hybrids between an acarus and a bacillus, an amœba and a crystal. On the other hand, clinical experience shows that psoriasis, for instance, may occur in a

case of syphilis, develop, and be cured quite independently of the latter; and that rheumatism and typhoid fever, gout and cholera, ague and pneumonia, are equally independent. The whole conception of complex diseases belongs to a time when each was regarded as a separate living thing, and is altogether out of place when we look upon them as disturbed physiological conditions.

Age and Degeneration as Causes of Disease.

To go back once more to the ovum, or organism in its simplest form, let us now suppose that it has inherited nothing but physiological, that is, normal structure and tendencies; that it has met with no external disturbance, or, as we express it in medicine, is free from congenital and acquired diseases. Nevertheless, we know that it will not last for ever. One diathesis which was common to all its predecessors it has undoubtedly inherited, and this fatal heritage is a disposition to die. Its period like theirs is limited, and when its natural term is reached, it too will cease to perform the functions of a living being, and its protoplasm will become subject to the reactions of common chemistry. Why either a cell or a more complicated vegetable or animal organism should wear out, is not easy to explain; in the more perfect animal mechanisms with which we are best acquainted, their construction and their working are so admirably contrived, so delicately adjusted, that while on the

one hand we wonder that so fragile and exquisite a piece of mechanism should escape the various accidents of life, or, still more wonderful, throw off the effect of injuries, we are equally at a loss to say why, without external accident or assignable cause, the machine should in time wear out and come to a standstill. If no external injury befall the body, no poison, no bacillus, no parasite approach it; if expenditure of force be restrained to the narrowest and most selfish needs of the individual, and the income of food be limited in proportion, there are yet certain expenditures which must continually go on. The diaphragm and the heart are muscles which have no prolonged holiday like those of the limbs during sleep; yet they also have their intervals of rest, no less constant than of work. Nor does it appear that the musculo-nervous apparatus or the secreting organs ever cease to act by mere weariness of having toiled for fourscore years. Degeneration does not seem in any case to be what has been called a mere exhaustion of the vital powers, or using up of the first impulse which was given at conception, in the same way as a moving body gradually comes to a stop by friction, or a burning mass is quenched when the fuel is consumed. On the contrary, we can, I believe, always refer death from old age either to incidental diseases of external origin, injuries, cold, or contagia, or else to structural changes in the apparatus, and particularly in the vascular apparatus. A continued hydraulic

strain upon the great arteries is a kind of mechanical injury which leads to a chronic inflammatory process and a gradual destruction of their elasticity and power of resistance to pressure. Disease of the arteries and failure of the vascular supply to the brain are the chief degenerative changes which lead to senile death.

That old people are far more liable than young to succumb to accidental disorders, such as diarrhœa, bronchitis, pneumonia, and enteric fever, may be to some extent explained by the degenerated state of their arteries, the frequently emphysematous condition of their lungs, and the probability that one or both sides of the heart have become dilated owing to one or both of these causes being present. If to these frequent states of structural decay we add the chances that the liver is more or less fibrous and atrophied, the kidneys more or less granular, the pleura more or less adherent, as the result of previous accidental diseases, we shall perhaps have accounted for the great majority of senile deaths without needing to invoke a gradual decay of what is called vital force.

Deaths which are ascribed to old age may be the result of chronic diseases which chiefly affect the later period of life, such as cancer, granular degeneration of the kidneys, bronchitis and emphysema, apoplexy from disease of the cerebral vessels, and various forms of structural disease of the kidneys and bladder from enlargement of the prostate.

Or the disease may be an accidental one to which all ages are liable, but the power of resisting it is diminished.

In this last respect the two extremes of life resemble one another. The freshly lighted taper and that which is burnt down to its socket are both easily extinguished by the slightest puff of wind. Hence the mortality of typhus, variola, and enteric fever in persons over sixty; hence the apprehension with which we regard pneumonia in an elderly patient.* The most common causes of senile mortality are the same as those which are fatal among infants,—diarrhœa, particularly in the summer months, and bronchitis, particularly in the winter. To these trivial disorders an old man succumbs as he does to an accidental injury—to a fracture of the thigh or of a rib. When none of these external disturbances have occurred, when the long-drawn thread of life is cut at last suddenly during tranquil sleep or in the midst of placid conversation, the cause is probably always syncope. It is the heart and blood-vessels which give out at last.

We must, it is clear, admit that old age is pecu-

* I have, however, been astonished to see patients of seventy-five and upwards recover from acute bronchitis, acute pleurisy, and lobar pneumonia. We may perhaps explain such exceptions by the consideration that persons live to fourscore years by reason of strength. They must have good hearts, kidneys, and power of resistance, or they would not have lasted so long. To some extent all persons over seventy may be regarded as picked lives.

liarly vulnerable and unable to withstand injury. The cartilages are calcified, the bones are brittle, and the loss of fat diminishes the elasticity of the tissues and adds to the risk of shock. The lungs are light and thin from senile emphysema, the muscular strength is greatly diminished, so that there is less power in making voluntary efforts in respiration, and the nervous system is no longer sensitive and active, so that conservative reflex movements are sluggish, and the old man can neither save himself from a fall nor appreciate the deficiency of oxygen in the blood which reaches his brain. The loss of appetite prevents due nourishment, and the bodily heat is difficult to maintain.

The organs of sense are with few exceptions impaired, chiefly from demonstrable structural changes, but also from diminished excitability of the nervous centres. With respect to the mental powers, it is difficult to separate the results of intrinsic changes in the cerebrum from those due to deficient supply of blood; in true senile dementia there is probably always arterial hæmorrhage or obstruction, or wasting of the cortex. Loss of memory appears to be the only mental symptom which is constant after a certain age. We have numberless examples of high intellectual powers, of beauty and dignity of character, surviving physical decay, but whatever may be the clearness of judgment, the eloquence of language, and energy of will in an aged body, it would probably be found without exception that

such mental exertion can only be maintained for a short time.

When

> "The mellow glory of the Attic stage,
> Singer of sweet Colonus and its child,"

was obliged to vindicate his mental capacity in extreme old age, he did so by reading the chorus that he had just composed, celebrating the beauties of his native place*—and the vindication was triumphant. But probably Sophocles did nothing more that day.

Overwork and the Strain of Modern Life.

A question suggests itself whether "senile" or seniliform degenerative processes and the diseases they produce may be the result of increased wear and tear at a comparatively early period of life. Is premature old age with its attendant diseases produced by too heavy taxing of the bodily machine? Is "overwork" a cause of disease?

To this question we may, I believe, broadly answer No. The vanity of human nature is tickled by ascribing its disorders to such respectable antecedents as industry, energy, and intellectual activity. We must all have felt this when the obvious results of habitual gluttony are ascribed by a patient or his wife to an "overtaxed brain" or too strenuous devotion to business, especially in the public service. There is no fear of any one of us using our brains

* Oed. Col., v. 668, *et seqq.*

too much for our health; nor do I believe that any amount of mental labour or business or honest work of any kind interferes with health or shortens life a day; even if it did, who would not rather be worn in use than rust in idleness? Which of us would not choose a short span of life, filled full with action and with thought, with sorrow and with joy, with effort and endurance, of helpful service to others, and enjoyment of living oneself, rather than to wear out a tedious existence of monotonous and selfish ease? There is, I repeat, no reason to believe that work in itself is injurious, even if it encroach on the hours of sleep. Nature will take care of herself, and irrepressible drowsiness will bring the needed rest if it be not driven away by artificial means.

The trite commonplace "that it is not work that kills, but worry," is just as false; what is called "worry" is commonly only ill temper. The evil results of the gambler's or the spendthrift's mode of life are indirect. Such patients go too long without food, and then eat more than they can digest; or they take stimulants from time to time to excite them for the moment, or sedatives to give the repose which they have not fairly earned.

Surely we need no proof beyond the example of the best men in our profession that intellectual work and constant employment of time conduces, when associated with an unselfish and benevolent temper and extreme moderation in all things, not

only to a happy and a useful life, but also to such length of days as was granted to our late revered and beloved colleague, Sir William Bowman.

Nor can I pass from this part of my subject without a word of protest against the assumption which has become a commonplace of newspapers and of conversation, that the present age, the close of this nineteenth century, is fuller than any other of what is called "worry" and anxiety, mental excitement, "overstrung nerves," and what not. It is gravely asserted that railways and telegraphs and the modern race for success are shortening life in Europe generally, and in England and America in particular, that there is an enormous increase of what are called nervous disorders. It is also stated that cancerous disease is much on the increase, but this, I am informed by patients, is due to the increased consumption of tomatoes. It is said that the "neurotic temperament" or "type" or "diathesis" is intolerant of the heroic means of combating diseases which were well borne by such dull-witted fellows as Steele or Johnson, by the comfortable, prosaic, easy-living men who saw the mutiny of the Nore and Napoleon's army gathered to invade England, who mourned for Nelson and for Moore, and exulted in Corunna and Trafalgar.

Surely we are complimenting and pitying ourselves without cause. What are the hardships of a railway journey to Bristol or to Edinburgh, compared with those of a journey by coach a century ago? What

are the trivial anxieties of rates and taxes and board-school elections, compared with those of previous generations, when few men had not been robbed on the highway or otherwise put in personal peril, and when the citizens of many towns had experienced a siege, a famine, a pestilence, or a massacre? We may be sure that losses of money, family misunderstandings, disappointments and annoyances, bodily aches and pains, bills that cannot be paid, and accounts that will not balance, were just as common a hundred years ago as now—in the days of Horace or of Lucian as in those of the 'Spectator' and the 'Rambler.'

That life is in every way easier than it was, a little reflection will convince us; and perhaps there is no better illustration of the fact than to consider the annoyance, cold, and vexation which must have been constantly endured while trying to strike a light with a tinder-box, compared with the ease, certainty, and rapidity of the lucifer match, or the still more rapid, easy, and brilliant electric light.*

The complaints we now hear were just as common fifty or a hundred years ago; the nation has been going to ruin ever since it was a nation; the present generation has always been degenerate, more feeble, more vicious, and more diseased than any which

* Let me recommend the perusal of a somewhat prolix, but not uninteresting book which obtained the honour of a review in the 'Edinburgh Review' by Sir Walter Scott, called 'The Miseries of Life.' It was published in 1806.

preceded it; but at the same time more sensitive, more interesting, more pitiable than any other—and above all, gifted amid the general decay with a few superior intellects which could appreciate and bewail the evils which they never tried to cure.

Hypnotism is not a more silly business than clairvoyance, nor that than mesmerism and phrenology, and the worst performances at La Salpêtrière are only reproductions of Mediæval cases of possession. If we may judge from plays and novels, fainting and the vapours are much less common in the nineteenth than in the eighteenth century. Men in general live a more domestic, healthful, and sober life than a hundred years ago; women are less nervous, and bear and rear larger families; while children, both rich and poor, are certainly better clothed, better fed, and grow up into taller, stronger, and healthier men and women.

LECTURE II

Disease as influenced by sex and age—Statistics—various sources of fallacy—Sex as a predisposing cause to certain diseases—Liability of the several periods of life to certain disorders—Climate and seasons, race and occupations as ætiological conditions of disease.

In my first lecture I proposed the statement that we are wrong if we seek for the cause of disease as of some monstrous and abnormal, almost illegal interruption of our comforts and our life; that diseases are part of the vast and mysterious, though beneficent system under which we live, that morbid processes are strictly physiological, and that the origin and conditions of disease and of death must be sought by the same methods as the origin and conditions of health and of life.

We must deal with ætiology as a branch of natural history,—and *Historia*, let us remember, signified inquiry as well as record,—to be studied in the light of comparative pathology and the great principle of evolution. I sought to direct attention from this point of view to the meaning of parasitic diseases, of injuries and irritants, of contagion, congenital lesions and degeneration; and referred to the operation of such general biological laws as reaction to stimuli, habit, tolerance and immunity, heredity and degeneration.

Statistics.

Of the two methods of inquiry into the laws of nature, observations and experiment, the latter has a much narrower field in the investigation of the origin than in that of the nature of processes which shorten life.

In passing from the general causes of disease, to the predisposing or protective influence exerted by such conditions as the *sex and age* of the subject, the race to which he belongs, the climate in which he lives, his occupation and his habits, we enter on a branch of ætiology where physiology and general biology help us little, and we have to depend chiefly on statistics, *i. e.* on the collocation in large numbers of instances from the study of which we hope to arrive at general conclusions.

Here if we would walk surely we must walk warily, for the pitfalls are many.

Let me offer a few examples. It was said by a biographer of the great Dr. Thomas Young that although his scientific reputation was far higher than his medical, yet it was a remarkable fact that the percentage of deaths among his patients at St. George's Hospital was much lower than among those of his colleagues. Doubtless the explanation is that fewer acute cases found their way to his wards than to those of more zealous and enterprising physicians.

By the returns of the Lying-in Charity of Guy's Hospital, extending over several years, it was conclusively proved that more women were brought to bed of their second child than of their first. The inexperienced primapara often did not seek the skilled assistance which she afterwards took care to secure.

If any disease is found to be as frequent in elderly people as in children, it is clear that they are not equally but far more liable to it, since many children do not reach old age, but every old man or woman has been a child. Quite apart from incidence on one age or another, the cases of death from any disease will be less frequent late in life than early, because there are always fewer to be ill of it.

The superiority of the treatment of enteric fever by cold baths was once asserted on the strength of the very low mortality among patients so treated in the Military Hospital at Munich. But it was forgotten that with picked men, free from previous disease, well fed and cared for and under strict discipline, at a very favourable age, put early into a ward, *any* treatment would be likely to show better results than in cases taken from the population at large.

Again, more male than female children come into the world; hence, for the first few years of life, the proportion of the deaths of boys will be greater than that of girls, other things being equal. The

disproportion is gradually diminished and then gradually reversed, until after fifty the number of women dying from *any* given disease will, other things being equal, be greater than that of men.

Statistics, to be of value, must be taken without selection, designed or unintentional; they must refer to the same things, *i. e.* in our case to well-defined and easily recognised diseases; they must be in sufficient number to compensate for such accidents as a "run" of "heads" before a "tail" appears; and they must be carefully "corrected" by other statistics of the number of persons available for each enumeration.

For example, in order to judge of the success of an operation, say abdominal section for intestinal obstruction, statistics are often collected from the medical journals and the transactions of societies. But such results are far too favourable, since everyone naturally makes known his successes more readily than his failures. Again, statistics of asthma collected from various sources are worthless, for they will include cases of emphysema, of cardiac disease, of anæmia, of phthisis, and of hay-fever, as well as those of primary spasmodic asthma. Statistics of cancer will yield contradictory results according to the latitude given to the term, according to the basis being clinical or anatomical, according to the sex, age, and circumstances of the patient,

and according to the diagnostic or pathological skill of the recorder.*

Another example of a statistical fallacy is, I believe, afforded by a prevalent belief that cancer is increasing among the Jewish community, and also in New Zealand. Jews are longer lived than Gentiles, and New Zealand is one of the healthiest climates. Therefore in both cases there will be the more persons in proportion to the entire population who have arrived at the age most open to malignant disease, than there are in less favoured populations. We must all die of something. A low infant mortality leaves a larger number to die of drink, exposure, and accident, in adult life. And a low mortality between twenty and fifty due to temperance, sedentary habits, and care of health, leaves a larger number of persons between forty-five and seventy to die of cancer, apoplexy, and other diseases most prevalent in the latter third of life.

In once trying to ascertain the truth of a statement that pleurisy in general and empyema in particular is much more often on the left side than on the right, I found a long run of cases of leit empyema, and it was only after about fifty that the inequality began to diminish and at last disappeared. Statistics are fallacious unless large numbers can be obtained, and certainly we ought never to talk of percentages when our cases fall short of a hundred.

* Dr. Newsholme has to my mind conclusively proved that the supposed increase of cancer among the population of this kingdom is due to increased recognition of the disease in internal organs.

Lastly, all statistical statements must be controlled, just as much as experiments. A series of rabbits are fed with some microphyte, and subsequently many of them are found to have *Pentastoma tænoides* in the liver, but the significance of the fact is completely altered when we find that it is common in rabbits which have not been meddled with. Take an equal number of rabbits who have not been so fed, and we find that the same parasite is present in about the same frequency. Or we inject lactic acid into a cat's veins and find after a time that the mitral valve is thickened; a proof of the origin of rheumatic endocarditis which holds until we take another cat who has not been injected and discover that it too has the same anatomical appearance in the valve. So in like manner it has been stated that persons with light hair and blue eyes are predisposed to rheumatic fever, and no doubt the majority of our rheumatic patients have light hair and blue eyes; but this is equally true of patients with broken legs, since the majority of our population in England is xanthocroic. Ground-down teeth are stated to be a proof of gout, but gout belongs to later life, and persons in later life who have never had gout have the teeth ground down by use.

Sex as a Predisposing Cause of Disease.

With respect to the ætiological influence of sex, several interesting facts show the wide-reaching dif-

ference between men and women in their liability to diseases, exclusive of those directly connected with the uterus and ovaries, with menstruation, pregnancy, and lactation. Some of these sexual peculiarities may be explained by the indirect effect of primary disorders of the strictly female organs; others by the more emotional tendency, the greater liability to disturbance, and the smaller power of self-restraint present in the majority of women compared with the majority of men. Others, again, may be referred to the effect of the more exposed and hazardous life lived by men, to their different physical and mental education, and to their greater liability to the evil results of gluttony and of drink.

Under the first head we may account for the greater frequency of cancer of the breast in women compared with men; for a certain number of cases of cardiac disorder dependent on the high tension of the blood, the "physiological plethora" of pregnancy; for a larger number of cases of hydronephrosis and consecutive chronic Bright's disease, which result from prolapse of the uterus; for the majority of cases of severe anæmia with constipation; and for perhaps a third of the cases of gutta rosea which depend on disordered menstruation or on the menopause; also for the rarity of gonorrhœal synovitis in women, and the frequency of pelvic peritonitis.

Under the second head we may rank the prevalence among women of functional nervous disorders

to which the adjective hysterical has, not without reason, been applied; with comparative rarity of the more fixed and permanent fancies to which (also not without reason) the title hypochondriac has been given; also the frequency of cardiac palpitation and other functional disorders of the heart, including perhaps that most singular of maladies, exophthalmic goitre; and the uncertainty of both symptoms and event which attends cases of paralysis among women.

Under the third head, that of habits and mode of life, fall the proclivity of women to the various forms of headache and neuralgia, constipation and dyspepsia (including that due to the abuse of tea-drinking), obesity and anæmia, from want of fresh air and active exercise; as well as their comparative immunity from atheromatous disease of the heart and arteries, particularly from aneurysm and from aortic (or "sigmoid") valvular lesions, from delirium tremens, cirrhosis of the liver, and gout; from hernia, volvulus, and other forms of intestinal obstruction which depend on strangulation of previously healthy gut, compared with their liability to those more chronic forms of obstruction which are caused by adhesions about the pelvic organs.

The greater frequency of cancer in women than in men* depends apparently on two causes, the first

* The numbers given by the Registrar-General for the thirty-three years ending with 1880 of deaths from cancer in the two sexes were, men 84,913 and women 187,735.

and more important being the great liability of the mamma and uterus to cancer, and the second the comparative freedom of women from many of the preventable diseases of men, and the consequent larger number of those who are alive at the age most liable to cancer.

The several effects above mentioned—the direct working of uterine and ovarian disorders, the ready susceptibility of the nervous system, the comparative absence of exposure to weather and accident, of muscular labour and strain, and the diet, clothing, and habits which civilised life impose upon women; all of these have their full play during the time of ovarian activity, and are much more restricted before and after that middle period of life, in girls under puberty and in women after the menopause. We do not find that the specially female maladies are nearly so common in childhood or in old age. On the other hand, we do find that men of feminine impressibility suffer from hysterical and neurotic affections, from palpitations and syncope, or, to use our fathers' terms, young and emotional men are liable to "the vapours."

There still remain instances of marked sexual proclivity without any plausible explanation.

Male children are more liable than female to tuberculous meningitis, laryngismus stridulus, and tetanilla. And in patients of all ages, most structural diseases of the nervous system are decidedly more frequent in men than in women. I do

not refer to apoplexy and hemiplegia because, so far as these clinical diseases depend on cerebral hæmorrhage, they are not nervous but vascular in origin; nor to tetanus, because that is an infective and traumatic disease, like pyæmia. But why should cases of acute myelitis, Cruveilheir's palsy, bulbar paralysis, and Duchenne's pseudo-hypertrophic paralysis, of tabes, of cerebral tumours, and general paralysis of the insane, be so far more common among men than among women?

Cerebral abscess (according to Dr. Pitt's statistics which he brought before the College in the Goulstonian Lectures last year) is much less common in women than in men, apart from its occasional traumatic origin. This last cause cannot explain the preponderance of male patients with cerebral tumour, since Dr. Hale White has found that the same preponderance applies to male children and to tubercular growths. Why, again, are the atrophic forms of paralysis so far more common among males, and yet the essential acute spinal paralysis which is known as distinctively children's so impartial between the sexes?

The nervous affections in which the female sex is more or less predominant are: hysteria, including hystero-epilepsy, syncope, and palpitation, some forms of emotional insanity, the eclampsia of pregnancy, and puerperal mania, all of which are ætiologically related to female physiology. Megrim or

sick headache and neuralgia of the fifth pair of nerves are certainly more common in women than in men, while the opposite is the case with sciatica —a hint perhaps that it is in most cases a neuritis, not a true neuralgia.

Chorea is more common in girls than in boys under puberty. Thus, of first attacks under sixteen, under my own care, there were thirty-four in boys to seventy in girls, or 1 : 2; if all attacks were counted the contrast was still greater. Chorea is far more rare in grown men than in grown women. My cases between sixteen and twenty-six are thirteen in men and twenty-six in women.

This relative persistency of a children's malady in the female sex is an interesting point. For comparative morphology teaches that the male organism is more differentiated than the female, further removed from the embryonic condition; hence we should expect that the pathology of the adult female would approach more nearly than that of the adult male to the pathology of the child.

In a somewhat similar way we may "explain" (*i. e.* refer to a wider group of facts) how some of the diseases of women after the menopause approach nearer to those of men,—as, for instance, gout.

To complete the list of diseases in which the female sex operates as a powerful predisposing or favouring cause—not certainly as the efficient cause—we may mention myxœdema, bronchocele—endemic and exophthalmic,—gall-stones, acute yellow atrophy

stones, acute yellow atrophy of the liver,* moveable kidney,† chlorosis, osteo-arthritis (particularly when occurring early and affecting the fingers), mollities ossium, sclerodermia, and xanthelasma.

Among those to which the male sex predisposes are plastic bronchitis,‡ hay fever, asthma, angina pectoris, atheroma with aortic valvular (sigmoid) disease, cerebral hæmorrhage and aneurysm, typhlitis,§ invagination of the bowels (but in children only, not in adults), intestinal strangulation generally, but particularly and almost exclusively that due to the presence of a *diverticulum ilii*, hepatic abscess, paroxysmal hæmoglobinuria, diabetes,‖ Addison's disease of the adrenals, hæmophilia, gout, and the severer forms of anæmia.

Of diseases due to bacterial, parasitic, and fungous invasion, few show any ætiological relation to sex. Such exceptions as tetanus, glanders, and anthrax are, like lead-poisoning and grinders' phthisis, sufficiently explained by the greater exposure of men to opportunities of contracting them. But, on the whole, smallpox and scarlet fever, typhus and cholera, hydatids, scabies, and ringworm are impartial in their incidence, and find equally suitable soil in either sex. One exception appears to be

* 88 : 55 (Thierfelder). In eleven cases at Guy's Hospital six were female and one male.

† 97 : 10 (Landau). 92 : 8 (Stefler).

‡ 42 : 13 (the late Dr. Peacock and cases of the writer).

§ 24 : 9 (P.-S.).

‖ 2·45 : 1 (Dr. Pavy's private cases).

whooping-cough, which is either less common or less fatal among boys than among girls.*

With respect to two important diseases which are generally believed to be much more frequent among women than men, I believe that there is no such marked difference between the sexes. One is gastric ulcer. This, though no doubt common in young women, is also common among men of all ages. The following statistics are remarkable in this respect. Brinton and Wilson Fox make it more than twice as frequent in women as in men, and v. Ziemssen's numbers (confirmed by autopsy) were 35 to 15. Of 100 cases collected from patients during life, I found the proportion nearly the same, 67 in women and 33 in men. But taking 101 cases of gastric ulcer found after death, I was surprised to see that there were 59 in men, and only 42 in women. It may be that we allow too great weight to the fact of sex in making a clinical diagnosis; or it may be that cases of the disease come earlier under our notice in women; or, again, that they are more amenable to successful treatment by restricted diet and confinement to bed.

The second disease is the essential, primary or idiopathic form of anæmia which was fully described by Addison in 1855, and re-discovered thirteen years later by Biermer. This physician,

* During the years 1848–87 the Registrar-General returned 192,190 deaths from whooping-cough in males, and 233,599 in females.

working at Zürich, Immermann at Basel, Quincke at Bern, and Lebert (whose account was long subsequent to that of Addison, but some time before Biermer), agreed in stating that what they called "progressive pernicious" anæmia was far more common in women than in men, and the statement was naturally confirmed by Professor Gusserow from a gynæcological point of view. A study of their cases, and of those collected by Eichhorst in his monograph on anæmia, shows that some of them would, in England, be called chlorosis, and others would be regarded as anæmia secondary to hard and scanty fare, gastric disturbance, diarrhœa, hæmorrhage, or lactation. On this principle we might add cases of "pernicious" anæmia in both sexes due to cardiac or renal disease, plumbism or cancer, and, as a matter of fact, such cases have been admitted by certain writers. The important fact discovered by Addison has thus been lost sight of—that patients may die of anæmia which is *idiopathic*, primary, essential.

If we exclude the secondary and spurious cases, and keep to his masterly definition of the disease, we shall find that it is quite as common among men as women. And this agrees with its true pathology; for it is quite distinct from chlorosis, and is naturally allied with the other essential and grave forms of bloodlessness which we know as leuchæmia and Hodgkin's disease, both of which are more common in the male than the female sex.

In the thirty years which followed Addison's death (1859—1889) there were 21 men and 15 women in Guy's Hospital who died of idiopathic anæmia. Among 107 cases collected from various authentic sources and all verified by autopsy ('Guy's Hospital Reports,' 1882), I found 48 in men and 59 in women. This differs much from Eichhorst's numbers, 30 men to 65 women; but if we use the information he has carefully given of his cases, we can exclude many as not primary anæmia, and the numbers would then stand as 11 men to 12 women. That this is the truer proportion is proved not only from the numbers given above, but also from those collected by Dr. Coupland in his Lectures on Anæmia before this College in 1881 (56 men to 54 women), and also from those collected by Dr. Musser, of Philadelphia (24 men to 15 women).

Age as a Predisposing Cause of Disease

The ætiological relation of *age* to diseases is interesting, but in many cases obscure. In some respects childhood and second childhood agree, particularly in their liability to death from bronchitis and from diarrhœa. This argues less power of resistance than in the adult, although both children and old people sometimes endure operations remarkably well. In both extremes of life there is difficulty in maintaining animal heat. One of the most practical points to attend to with very young or very

old patients, but particularly in abdominal complaints, is to keep the extremities warm.

In young children there is a preponderance of reflex action, the spinal system being active, sensitive, and unembarrassed by cerebral interference. Hence the prompt and marked character of the superficial reflexes (plantar, palmar, and cremasteric) in children, their ready laughter when tickled, their frequent and easy vomiting, their liability to convulsions. With the same physiological condition we may associate the prevalence in children of tetanilla, carpopedal contractions, trismus, and laryngismus stridulus, and the characteristic frequency of infantile convulsions, particularly those which take the place of rigors in an adult. To the same activity of reflex action we may perhaps ascribe the remarkable frequency of invagination of the intestine in children.

Again, children are dependent for their food on others, and hence are liable to suffer from an insufficient or ill-constructed dietary. This is probably the cause of rickets, and also of infantile choleraic diarrhœa, of so-called dysentery, of infantile scurvy, and of marasmus.

The peculiar liability of children to certain forms of atrophic (including pseudo-hypertrophic) paralysis has not, I believe, received any explanation. Some of these are of spinal, others of muscular origin, some inflammatory, others vascular, and others, again, developmental in origin.

The predominance of children among those suffering from epidemic specific fevers is no doubt due to most adults being protected by a previous attack. When smallpox or measles invades a virgin population, all ages are indiscriminately seized.

One common character of children's diseases is that they are generalised in distribution. Tubercle invades a greater number of organs. Phthisis is less confined to the apices, pericarditis and endocarditis are more constantly associated, and diseases of the skin keep far less definitely to their usual localities.

Ætiology of Climate, of Race, and of Habits

The effects of *climate* as an element in the causation of diseases are doubtless important, interesting, and instructive. But exact accounts of endemic diseases and of local modifications of diseases are in most cases still to seek.*

Among the few exceptions to the general uniformity of geographical distribution, which we observe in our own islands, may be mentioned the frequency of osteo-arthritis in Ireland (to which we are indebted for the admirable work of Adams), the rarity of hydatids and the comparative frequency of favus in Scotland, the absence of malarial diseases in Ireland, the prevalence of urinary

* I cannot but refer in this connection to the valuable reports from the medical officers living in the Chinese ports which have been drawn up and published by the wise forethought of Sir Robert Hart.

calculus in Norfolk, and of endemic bronchocele in Derbyshire.

One of the difficulties of geographical pathology lies in separating the effects of climate from those of *race* and manner of living. That the marked racial differences between, for example, Europeans, Negroes, and Chinese, affect their functions as well as their mental character, and their reaction to disease as well as their normal physiology, there can be little doubt. But on the whole it seems probable that even in such extreme cases the differences are but small. The surprising lack of sensibility to pain which is characteristic of many races seems rather to be due to certain mental qualities being undeveloped than to ethnological peculiarity. Negroes are often said to be less subject than Europeans to malaria, but a statement which I once made to this effect has been since corrected by those who speak with the authority of personal experience. At present we may, I believe, state that there is no infection against which any race of mankind is immune, that there is no disease peculiar to any nation, or from which any nation is exempt, and that there is no drug known, whether poisonous or medicinal, which fails to exert its ordinary physiological effects on every member of the human family.

The influence of *seasons* on diseases is well marked in the tropics, and even in our own temperate climate there is no doubt that, for instance, bronchitis and pleurisy are more prevalent in winter

and diarrhœa during summer and early autumn. But it is doubtful whether pneumonia is much influenced by the season or temperature; and, notwithstanding the general belief to the contrary, it is extremely difficult to find evidence that rheumatic fever is. If we compare the admissions for this disease to Guy's Hospital in different months with similar statistics at the London Hospital and in Paris, there does not appear to be any constant relation to cold or wet months.* At the same time we must remember that we have often mild wet weather in December, and dry bright weather in January, that there are cold and rainy days in summer, and that the free perspiration and thin clothing of summer are very apt to cause a chill.

Probably no one now concerns himself with Sydenham's doctrine of a certain epidemic constitution of each year, shown by the prevalence of a distinct type of disease. That so shrewd and practical a physician should have been thus led astray is a warning against premature generalisation and against so-called philosophical views in natural science. We now know that the prevalence of scarlet fever or plague of smallpox or cholera or influenza depends upon facilities of human intercourse. There is no reason to suppose a general proclivity to nervous affections, or to inflammatory diseases, or to diabetes, or to cancer, in any particular season. If there were, it would on analysis and investiga-

* See Appendix, table.

tion be no doubt found to depend on such association as that between hot weather and drowning, cold weather and scabies, the Parliamentary vacation and the growth of large gooseberries.

No one now believes in the "change of type" of disease which was gravely brought forward thirty years ago to explain why physicians and surgeons who had bled, purged, and blistered indiscriminately in the former half of their career, abstained from their remedies in the latter half with equal lack of discrimination. Diseases, if they have changed at all within historical periods, have changed very slowly, and owing to known changes of climate, of foods, of habits of life. But to imagine that there has been a "change of type" within human memory is a fond delusion, like that which finds our winters less bright and frosty, our springs colder, and our summers shorter and more chilly than they once were. In spite of variations backward and forward, there is every reason to believe that the climate of England has been the same since the Conquest. In each century the Thames has been frozen occasionally, and a perusal of the 'Chronicle' of Sir Thos. Baker, his predecessor, shows the same periodical recurrence of long frosts, very early and very late springs, with occasional hurricanes and earthquakes.* In winter "the fields are dank and ways are mire" as when Milton wrote to Lawrence.

* Lord Verulam caught a chill which proved fatal when he got out of his coach just before Easter, 1626, to try the effects of cold

The showers of April are as sweet now as in the days of Chaucer, and the glory of an April day is uncertain as in those of Shakespeare.

The importance of *occupation* as a predisposing cause of certain maladies is now well appreciated, but it depends in each case on liability to certain definite morbid agents, not in proclivity to disease in general.* Devonshire colic and painters' colic is not produced by living in Devonshire or by decorating walls, but by swallowing poisonous doses of lead; grinders' phthisis does not depend on anything but the power of dust thick and heavy enough to excite pulmonary catarrh, and thus to provide a soil for the invasion of an almost ubiquitous microbe. In this department ætiology has been most directly and obviously beneficial, and by its aid our profession has earned the blessing of the neglected and the poor.

in retarding decomposition of meat when the snow was lying on Highgate Hill.

Evelyn notes (March 27th, 1681) an extraordinary sharp, cold spring; not yet a leaf on the trees.

We sometimes have a May day (as in 1893 and 1894) which is as warm and flowery as poets ever sang, but for the most part our spring is cold and backward, and so it seems always to have been. Horace Walpole writes: "The spring is set in with its usual severity," and Cowper to the same effect. Peter Pindar (Walcott) writes—

"Hail, smiling May—and didn't it?"

and Hood—

"Come, gentle spring; ethereal mildness, come!
O Thomson, void of rhyme as well as reason,
There's no such season."

* On this subject Dr. Arlidge's work is a storehouse of well observed, collected, and digested facts.

LECTURE III

Special ætiology—Specific fevers—Gout and rheumatism—General and local diseases—Distribution among organs and tissues—Symmetry—Diseases of the nervous system—Cardiac diseases—Pulmonary diseases—Gastric, hepatic, and renal diseases—Cutaneous diseases—Conclusion.

Mr. President and Gentlemen,—Having thus far considered the general causes of disease, or, to speak more suitably to our imperfect knowledge, the conditions under which diseases arise, I propose to occupy the remainder of our time by considering the application of the general rules of ætiology to the certain case of particular disorders, and we will begin with the

Ætiology of Specific Fevers

This may be looked at from two points of view. Many of the "causes" to which, for instance, typhus was formerly ascribed really meant opportunities and facilities for coming in contact with its contagion. When men were crowded together in prisons, in camps, and in besieged towns, then it was that the typhous contagion, which is very active but readily dispersed by diffusion, had its best opportunity, and an accidental case, which would rarely spread in a large and well-ventilated house or infirmary, became the pestilence called gaol and

camp fever. For similar reasons enteric fever is peculiarly the disease of towns, since there drinking-water is most likely to be defiled from drains and cesspools. The fact that the contagion of Oriental plague is peculiarly tenacious in houses probably explains its recurrence shortly after a previous epidemic, and its disappearance after a town had been burnt down and rebuilt. The remarkable fact asserted by Caius and other respectable historians[1] that the sweating sickness particularly attacked Englishmen, even though they resided abroad, is less inexplicable if we remember first that it was certainly prevalent in foreign countries, and indeed gained its first entrance into England in the foreign army of Henry VII; and secondly, that English merchants in Flanders, Spain, or Italy would be far more likely than their neighbours to come in contact with persons and goods from England. The diffusion of cholera is no longer inexplicable if we accept the conveyance of its contagion from one person to another by means of water. The close connection of erysipelas with certain dwellings or with certain apartments in them, or even with certain beds, and its ready appearance when open wounds are present, depend upon the physiological characters of the micrococcus which is, or perhaps we should rather say which produces, its contagion. The age at which syphilis is most common and its

[1] Αἱ *μεταβολαὶ τῶν ὀρέων* (seasons) *μάλιοσα τίκτονοι νοσήματα.*—Hipp. Aph., iii, 1.

ordinary local seat are sufficiently explained by its most frequent method of conveyance. The virus of tubercle again, we may believe, affects the lungs more often than any other organs because it is persistent in a dried form, and readily inhaled with the air; while the prevalence of tuberculosis of the abdomen in children may probably be referred to the large amount of milk which they take. Glanders will obviously be rare except in the case of men who have habitually to do with horses; anthrax is confined to those who handle wool or hides; just as the frequency of tapeworm depends on the habit of eating uncooked or imperfectly cooked meat, and the occurrence of hydatids on the close companionship of dogs.

The rarity of Measles and Scarlatina in adults is justly attributed to the frequency of these disorders in children, whereby they are (with few exceptions) protected for the rest of their lives, and this is confirmed by their indiscriminately attacking all ages when they invade a virgin population. There would appear, however, to be a general insusceptibility to the invasion of specific microbes and to febrile reaction of whatever cause in adults compared to children, and in old age compared with middle life. We may, perhaps, associate this fact on the one hand with a slow and progressive atrophy of the thymus, the tonsils, Peyer's patches, the spleen, and other lymphatic organs as life goes on; and on the other to that

mobility of the nervous system which renders functional rigors, eclampsia, syncope, and spasmodic diseases generally more frequent in children than in adults.

Apart from age, special predisposition to receive a morbific virus may probably exist in many cases, but is extremely difficult to prove. Certain families are believed to be particularly liable to the invasion of enteric poison, others to scarlatina, and others to erysipelas. But the facts are probably explicable by concurrent exposure or by accidental coincidence.

Although it is certain that a temporary or permanent condition of the soil undoubtedly favours the development of the invading germs, yet we can seldom surmise, much less prove, how their first entrance and subsequent survival and multiplication are facilitated. Thus it is quite certain that relapsing fever (the *Hunger-typhus* of the Germans) is seldom or never seen except as the result of famine, although when once established it may be transmitted to well-nourished persons. If universal belief and medical authority count for anything, we should also admit that fear is an important predisposing cause of the Oriental Plague. In the case of Enterica there is better foundation for the belief that its development is favoured by the common diarrhœa which often precedes it, and that diphtheria may similarly be more apt to affect fauces which are already inflamed; while it is

certain that the micrococcus of Erysipelas finds a much readier entrance through a wound than in any other way. The bacillus of tubercle would seem to have little chance of making good its entrance in a healthy organism; and it is greatly aided by hereditary or acquired vulnerability, and in particular by a condition of pulmonary catarrh at the time. Cholera also, from the large numbers of persons who escape the disease even when it first invades a district, needs, one would suppose, some preparation—possibly an impaired power of digestion in the gastric mucous membrane, possibly abrasion of the intestinal epithelium—for the reception of the specific vibrio into the lymph-spaces and the lacteals. On the other hand, typhus, smallpox, and syphilis are such sure and powerful contagia that they force their entrance into the most healthy organism, baffle the most active phagocytes, and overrun the citadel of life without any aid from within.

Gout and Rheumatism

The ætiology of *gout* has in its broad outlines been apparently settled by the concensus of popular and professional opinion, from the times of Lucian to the present day. That intemperance in liquor in the sufferer or his parents is one, and probably the most important, cause of this disease may be readily granted. But there still remain questions under what circumstances drink is most likely to produce

gout, and what other conditions may develop it apart from drink. In speaking of gout, I mean recurrent attacks of inflammation accompanied by the deposit of urate of soda in the articular cartilages. That there is a gouty diathesis, or rather, to use Latin instead of Greek, a disposition to gout, I admit. A man whose father and whose mother's father had this disease is more likely to have it than another; a man who has suffered from gouty arthritis once is predisposed to a second attack. But this is only like the special liability of a child to tubercle, of one whose father had syphilis to corneitis, or of those who go to sea to be drowned. What was meant by the old term diathesis was that a certain proclivity to this or that disease was associated with certain definite marks or symptoms, and that by these symptoms one could recognise the proclivity and predict the disease or take measures against it. Of this, I venture to think, there is not a shadow of proof, and the word, along with all its vague, unscientific, and misleading associations should be transferred to the chapter of antiquities which deals with the Galenical humours, the temperaments, and dyscrasia of the blood.

The hereditary nature of gout is undoubted. It is far stronger than that of cancer, and perhaps stronger than that of phthisis; but there are many cases of gout which are not hereditary. How are these acquired? Not all drink will produce it, for

it is rare in Scotland. Not all wines will produce it, for it is rare in Spain and Italy. Not all malt liquors will produce it, for it is rare in Vienna and Munich. Difficult as it is to explain the fact, this strange disease appears undoubtedly to belong to the most civilised—possibly over-civilised—times and nations, to the stronger sex, to the most vigorous period of life, to conquering races, to the higher classes of society, and to the most able of the community. The truth of the reflection with which Sydenham comforts himself under the pangs of this disease has been confirmed by every succeeding generation :*

"At vero (quod mihi aliisque licet, tam fortunæ quam ingenii dotibus mediocriter instructis, hoc morbo laborantibus solatio esse possit) ita viverunt atque ita tandem mortem obierunt, magni reges, dynastæ, exercituum classiumque duces, philosophi, aliique his similes haud pauci."

No doubt the disease is far from being the exclusive appanage of rank and wealth. We see abundance of gout among our out-patients in London. But in them also it is hereditary, in them also it follows indulgence in strong drink, in them also it seems to choose the most vigorous and energetic for its victims.

Apart from hereditary disposition and from intemperance, there is no doubt we must recognise saturnine gout. Usually, however, plumbism does

* 'Opera Omnia,' p. 416 : Dr. Greenhill's edition.

not act alone; gout from this cause appears to be rare in the North of England,* and in the cases that have come under my own notice it has generally, if not always, been associated with beer-drinking and albuminuria.

The relation of a fit of the gout to mental excitement or violent emotion seems too well attested for doubt; but there is no evidence that the disease was ever so produced, and the fact may be plausibly ascribed to inhibitory action upon the excretion of nitrogenous products. Such nervous excitation would then take its place with an attack of indigestion as the exciting or determining cause of arthritis. In the same light we must regard mechanical injuries which will often determine gouty inflammation in a certain joint. In fact, it seems probable that the special liability of the ball of the foot and the root of the index finger to gouty inflammation depends upon their exposure to sprains and other injuries.

In considering the ætiology of *rheumatism* we have the same difficulty as with gout in defining the term. Both are historically associated with the exploded humoral system of medicine, and no one now supposes that the one depends on a drop of peccant humour distilling into a joint, or the other on a cold, moist catarrh flowing into the limbs and muscles. But just as the epithet "gouty" is by many applied with an arbitrary

* See Dr. Thomas Olliver's Goulstonian Lectures.

license to bronchitis and eczema and dyspepsia, psoriasis and iritis, so rheumatism is a convenient phrase for every ill-understood pain. In Germany the term "rheumatic" is commonly applied to every kind of disease which is believed, on more or less arbitrary grounds, to depend on the influence of cold. It appears to me that "rheumatic" should be used only as the adjective of the disease known as rheumatism or rheumatic fever,—multiple synovitis with a definite clinical course, complications and sequelæ, a well-marked and important malady known as rheumatic fever or acute rheumatism throughout the civilised world. If it can be shown that an attack of tonsillitis, myalgia, urticaria, pleurisy, or pericarditis is related to rheumatic fever, we mark a real and valuable ætiological point by calling them "rheumatic." In the absence of such proof the epithet is, I venture to think, useless to our patients and mischievous to our pathological conscience.

The ætiology of rheumatism, as thus defined, is still very obscure. The general belief that it follows exposure is certainly supported by striking cases. I do not remember, however, to have ever met with rheumatic fever following exposure to dry, cold east winds, or to low temperature, as in a railway journey in winter. The most striking cases are those of men or women who get wet through and are unable to change their clothes. That some such relation to the weather exists seems

shown by the geographical distribution of the disease, which is only too common in Northern Europe and the Eastern States of America, but is rare in the Mediterranean and the tropics, in Canada, Australia, and the high, dry, and cold districts of America.

I endeavoured several years ago to see whether the incidence of rheumatic cases in different periods of the year coincided with prevalent cold or wet, and the results are shown in a tabular form (see Appendix). But I was unable to draw any conclusions from these figures, and Dr. Archibald Garrod, in his excellent monograph on the subject, comes to the same negative conclusion. Supposing that the connection referred to is a real one, there are many reasons which may prevent its detection.

Rheumatism is pre-eminently recurrent, and a comparatively slight exciting cause may bring on an attack to one who has a disposition produced by a previous attack. We ought, therefore, to collect only first cases, but that would much limit our numbers. Again, the history of hospital patients is proverbially untrustworthy, and the chill to which they ascribe their rheumatism may have happened a week or more before the attack. Moreover our climate is so uncertain that a man who is much subject to skyey influences, particularly if inadequately clothed, may be as effectually chilled with rain in July as in January. Even in hot weather people are apt to catch cold by throwing off woollen cloth-

ing and placing themselves in currents of cool air. During a wet month we often have few cases of rheumatic fever in our wards, and they are certainly often absent in the coldest time of winter. My own impression, subject to the obvious fallacies of such individual and limited experience, is that one case seems to bring another, so that sometimes we have none for two or three weeks together, and at another every new case is taking salicylic acid. The points in the ætiology of rheumatism which belong to the early age of the patients, and to its position as a sequela of scarlet fever and its undoubted hereditary character, are facts which at present we cannot connect with one another, and time forbids dwelling upon them. But I would desire to express my disbelief—or perhaps I had rather say unbelief—in the chemical theory of rheumatism, which ascribes it to the presence of lactic acid in the blood. This conjecture, originally thrown out by Prout, was reasonable enough as the vague guess of a chemical pathologist; but, since every endeavour to prove this theory has rested upon demonstrable chemical or logical fallacies, it is a great pity that it still cumbers the ground. Moreover we must remember that our exploded theories still walk the earth and do mischief to our patients. Probably most of us have been asked, as I have, whether lemons were not bad for lumbago, because rheumatism depends upon an acid in the blood.

The ætiology of one of the many diseases of the

joints which used to be confounded under the elastic title of rheumatism or gout is now completely cleared up. I refer to gonorrhœal synovitis, a disease which we now know to be always and exclusively the result of urethral blennorrhagia, and to be totally distinct from gouty, rheumatic, syphilitic, or pyæmic arthritis.

A fourth kind of multiple articular disease which Sydenham and Heberden described as "chronical rheumatism," and the great Irish surgeons Adams and R. W. Smith by the unfortunate title of "rheumatic gout," is obviously and widely distinguished from gout by the absence of deposits of urate of soda, and from true rheumatism by the age of the patients it affects, by its course and symptoms, and by the entire absence of inflammation of the heart. That osteo-arthritis, as it is called in our college nomenclature, regarded as an anatomical condition, may be the occasional result of rheumatic or gouty or gonorrhœal synovitis, must be admitted; but that it has in the great majority of cases no such origin, but is, on the contrary, the result of a definite clinical and pathological disorder, is also beyond dispute.

My colleague, Mr. Lane, has with much ingenuity referred many of the distortions of the joints in this disease to traumatic causes. This explanation best applies to the cases in which one joint alone or one limb is the seat of the well-known anatomical changes in question. Where a similar morbid

state of the joints is more general it has, I think, always been recognised that it is in part the result of slight and individually insignificant but numerous and long-continued injuries; in other words, that it is the result of wear and tear. It has all the characters of chronic inflammation, *i. e.* it is free from the four classical signs of pyrexia, and it leads to hypertrophy with new formation of connective tissues. From this point of view the disease is found, as we should expect, in the later periods of life, and in those who have been hardest worked. In fact, this arthritis deformans may, in its ætiology, be compared to arteritis deformans, or atheroma of the aorta and great vessels. This same condition of chronic deforming and hypertrophic arthritis is very common in the lower animals. Writing on the subject in 1874, I referred to a large series of specimens from horses showing the disease in very marked form, and these were in the joints of the feet, on which the stress of a horse's work falls.

But while fully admitting the importance of direct and obvious injury as an occasional and of long-continued work and strain as a frequent cause of osteo-arthritis, there is no doubt that the same anatomical condition often occurs when no such explanation is possible, and when, moreover, there has been no preceding definite attack of arthritis, whether gouty, rheumatic, gonorrhœal, or traumatic. Such cases may occur in comparatively

early life, long before degenerative changes have begun; they are more common in women than in men, and they affect the smaller joints of the hand before those more exposed to injury and pressure. Moreover the clinical feature of the disease, its course, and especially its progressive aggravation after the unfortunate patient is no longer able to perform even the ordinary movements of the limbs, prove that in its characteristic form it is of idiopathic origin; that is to say, it is not secondary to any other disease, and is not the direct result of mechanical or chemical causes. It is certainly ætiologically connected with climatic conditions, for it is most common in those most exposed to our English climate, and is probably more often met with in Ireland than in any other country; that is to say, it appears to be favoured by residence in a mild and rainy climate. I have seen striking instances of its being averted or checked or cured by removal to the south of France or to the east coast of England—the one warm, the other cold, but both comparatively dry. Whatever the immediate and determining cause of this remarkable disorder may prove to be, it is, I think, certain that the term rheumatic gout is misleading, and the hypothesis of hybrid origin which it favours is opposed to all experience and scientific medicine, fatal to advance in pathology, and practically mischievous as leading to slovenly diagnosis and inefficient treatment.

Our task as physicians is difficult enough as it is. If diseases were of hybrid origin and mingled pathology it would be impossible.

General and Local Diseases—Distribution of Diseases.

In passing from the ætiology of what are termed "general" diseases to those which we name local, I would observe that the distinction, like most others in medicine, is convenient and conventional rather than strict or scientific. The morbid states which we call plumbism, anthrax, and syphilis, are "general," because a single cause, a poison or microphyte or contagium, invades the body from without, and overspreads every organ. Such maladies are improperly called "diseases of the blood," for the blood is only the channel which conveys them; they affect the lymph by which they usually gain access to the blood-stream and the solid organs to which the circulation carries them. But there may be similar invasion from without which remains local: thus a poisoned wound may set up a local abscess or lymphatic inflammation, which is stopped by the merest lympharia; thus the bacillus of tubercle may be limited in its action to the skin or to a single joint, without even gaining free access by the circulation to the whole body, and yet the condition is the same in origin and nature to the imminent and therefore fatal invasion of pyæmia and acute tuberculosis.

Again, although some contagions, like that of typhus, appear to affect every organ alike, most general or universal diseases have marked predilection for certain organs. Just as the lead or arsenic or phosphorus carried throughout the body has little or no effect upon many of the tissues, while it powerfully affects others, so that its action can be recognised after death by the local changes produced; so most "general" diseases have definite localities which they particularly affect—the skin in variola, the throat and kidneys in scarlatina, the ileum, spleen, and larynx in enterica, the joints and heart in rheumatism.

On the other hand, what was a local malady may become general; a traumatic lesion leads to pyæmia, a local chancre to "constitutional" syphilis, chronic tonsillitis or bronchitis or enteritis to swollen lymph-glands, and then to general tuberculosis, carcinoma to general infection by the same channel, and sarcoma to more rapid and indiscriminate invasion of all the tissues by the blood-stream.

With this understanding, however, it is convenient to consider the local diseases which are and are not liable to become generalised.

Some of them are produced by local infection from without, as is probably the case with lupus and certainly with ringworm, anthrax, chancre, and warts.

But others own no such causation, and must at present be regarded as "idiopathic." Still even

these are governed by laws of which we can partly discern the operation; and one of the most important is the rule of local peculiarities in disease.

When morbid anatomy was first systematically studied by Rokitansky and his contemporaries, it was natural to make a list of all the known pathological processes and then to apply them to each of the organs and tissues. On this plan one would have to describe hypertrophy and atrophy, fatty, calcareous, and pigmental degeneration, inflammation, fibroma, enchondroma, and carcinoma, malformations, and the presence of animal or vegetable parasites, for every organ and tissue alike, for the parotid gland and the crystalline lens, for muscle and for tendon. Such a preliminary method was valuable from its thoroughness; but long before the complete schedule was applied to every organ, it became clear that the various morbid lesions are not indiscriminately distributed over the body. Each general process is modified by the tissue affected; some organs are prone to certain forms of disease, and are absolutely free from others; and in almost every case the common type appears under different local forms.

Thus, speaking generally, we may affirm that some organs, like the small intestine, the salivary glands, and the pancreas, are almost exempt from disease except by such specific processes as enteric or tuberculous ulceration, or mumps. We may

state that suppurative inflammation never befalls the brain, the lungs, the liver, the spleen, the kidneys, or the testes,—in fact, any of the solid viscera, except from infection or local injury, no doubt leading to direct infection; while, on the contrary, suppuration is a frequent and easy event in the case of skin, mucous membranes, connective tissue, and bone, although the latter parts are far from obviously obnoxious to infection. We may affirm that tubercular inflammation, while extremely apt to affect the lymphatic cancellous tissue of the bones, the lymph-follicles of the intestine, and the lymph-glands, as a primary process, is never seen in the great mass of lymphatic tissue formed by the spleen except by secondary infection; while, on the contrary, primary tubercle is very common in the brain, the testis, and adrenal body, where lymphatic tissue is almost absent.

With respect to tumours, I remember as a student supposing that chondroma, fibroma, lipoma, and the rest were just as common in one place as another, and was surprised to find that all the fatty tumours I saw were under the skin, and all the enchondromata connected with bone.

It was then, strangely enough, not suspected that carcinoma had any special laws of distribution. This was partly because histologists had not yet definitely separated true carcinoma from other forms of malignant disease; at present we all, I suppose, accept Waldeyer's doctrine that

"true" carcinoma only begins in an epithelial surface. This, however, is not all, for the epithelial or keratoid form has its own seats of predilection, distinct from the other kinds of cancer; and the same applies to the colloid and adenoid varieties. But the localisation of cancer is not only histological; for the mucous membrane of the small intestine, though of the same general structure as that of the rectum, is one of the rarest, as the latter is one of the most frequent, seats of the disease; and the acini of the parotid are as distinguished by their immunity as those of the mamma by their proclivity to cancer.

In the ætiology of this disease it is not surely without meaning that its favourite seats (irrespective of histological structure) are the lips, tongue, and throat, the œsophagus, pylorus, and rectum, the places where there is most mechanical friction.

Another remarkable law in the distribution of cancer is that those organs (as the uterus, the mamma, the stomach, the rectum, and the testis) which are the most frequent seats of primary carcinoma very rarely become affected by secondary growths; while the liver, the lungs, the lymph-glands, the bones, and the serous membranes, while constantly affected by secondary, are seldom the seat of primary cancer. This proves surely that secondary growths are not the result of a general dyscrasia or diathesis, but are strictly derived from

the primary one by direct transmission through veins and lymphatics, and that primary cancer is really homologous and not heterologous in its origin, autochthonous and not immigrant.

Another example of the influence of topographical as distinct from histological relations, in determining the locality of a common lesion, is that of *cerebral hæmorrhage*, which in the large majority of cases occurs in one limited region supplied by a single branch of the middle cerebral artery.

In the lungs, again, phthisis chooses the apices, and not only the apices, but the highest point of the inferior lobe; while pneumonia almost as constantly begins below and spreads upwards.

But it is in the skin that we find this principle of local predilection most strikingly exemplified, because here the histological characters are essentially the same. We may, no doubt, explain certain peculiarities as due to the greater abundance of sweat-glands or sebaceous secretions, or to the size and length of the hair, or to exposure to cold, or to friction. But there remain many topographical peculiarities which are at present inexplicable. The striking and opposite localisation of psoriasis and eczema, the scarcely less marked local distribution of erythema, of lupus, of lichen ruber, of erythematous lupus, of xanthelasma, and many other cutaneous diseases depends upon quite unknown ætiological laws. All we can say is that psoriasis, for instance, affects one elbow, because,

of all parts of the body, it is most like the other, and affects the knees because they are most like the elbows.

The mysterious ætiological factor *Symmetry*, of which so much has been written, may be reduced to two cases. In one the same affection is seen on both palms, both soles, over both olecrana, or both popliteal spaces, because the corresponding sides of the body resemble each other more closely than anything else in nature. Thus psoriasis is symmetrical because it affects the same kind of skin, and because there is a right and left elbow, in the same way as Bright's disease and phthisis are symmetrical because they affect renal and pulmonary tissue respectively.

The other case of symmetry in which a disease affects the surface of the whole body, has sometimes been held to prove that the disease is "constitutional," or has its origin in the blood or nervous system. But surely this symmetry is not of genuine ætiological moment at all; it is merely the result of widespread disturbances affecting a symmetrical organism, and such accidental bilateral symmetry would be lost in the case of a man with a distorted spine—it would be asymmetry in a turbot or a snail, and pentagonal symmetry in a star-fish.

We shall find many illustrations of local modifications of general morbid processes in some of the ætiological questions of the diseases of the several organs to which I now invite your attention.

Diseases of the Spinal Cord.

The ætiology of the vast, and still in many places obscure, domain of Nervous diseases is at present an almost unexplored path. We are too ignorant of the pathology of many of them; some are so obscure that those who have the greatest experience are the most diffident of their diagnosis until it has been confirmed by an autopsy, while for the numerous functional neuroses we have not this means of obtaining a safe, even if narrow, foundation for ætiological theories.

This at least is clear, that nothing but error and confusion can result from speaking of "nervous diseases" in general as inherited, or more common in either sex, at any age, or in any country; or to ascribe them to intemperance, hard work, or any other cause. For the group is not a natural one, and can therefore have no common ætiology. Some of the most obvious, frequent, and recognisable diseases of the brain—apoplexy and hemiplegia—are really diseases of the heart and blood-vessels; cerebral tumours, if we except glioma and psammoma, are only accidentally present in the brain; cerebral abscess is never a nervous disease, but is either traumatic or secondary to suppuration elsewhere; meningitis is tubercular, traumatic, or infective, but never nervous; and phrenitis does not exist.

So also of the spinal cord, we know that a large number of cases of paraplegia are due to tuberculous

caries and a smaller number to malignant disease of the vertebræ. Again, of the large and obscure group of functional neuroses, we must remember that, if we knew their true pathology, they might prove to be as unconnected as the convulsions of uræmia with those caused by a cerebral tumour.

We can associate a certain number of local spasmodic diseases with frequent and long-continued exercise of a certain set of muscles. Scrivener's palsy is the first and still the best known of this remarkable group. Tetanus we now know to be an infective disease, as was long ago conjectured by the late Mr Poland.* The natural allies of Chorea are not with shaking palsy and congenital spasms, but with rheumatic fever, tonsillitis, and valvular diseases. The ætiology of tetanilla and laryngismus stridulus will only be understood when we know more of the essential cause and nature of rickets. Some affections of the peripheral nerves hitherto supposed to be functional are now found to have an anatomical basis; thus, Bell's palsy depends on inflammation of the trunk of the facial nerve, and alcoholic paraplegia upon peripheral neuritis.

There remain the group of primary or essential structural diseases of the cord which agree in their histological characters, and apparently differ in their symptoms only by the same local process affecting different tracts and tissues of the organ.

* In the first edition of 'Holmes's System of Surgery.'

Of these, acute myelitis or inflammatory softening of the cord is of entirely undetermined ætiology. In its most marked idiopathic form it attacks the young and healthy, and so far resembles the acute primary inflammations of the lung and kidney. Like them it is generally ascribed to exposure to cold and wet, and such striking cases have been recorded of this connection that we can hardly doubt its reality, although we undoubtedly see cases of acute myelitis, of pneumonia, and of Bright's disease, as we do cases of rheumatic fever, which cannot possibly be ascribed to exposure. Many examples of the former kind have occurred at Guy's Hospital, and in every case with which I am personally acquainted the kind of exposure has been long-continued wetting of the lower extremities; never dry cold, as of the feet and legs in a long railway journey in the winter. Some cases of acute intrinsic paraplegia without exposure may be due to embolism or thrombosis, but I never met with paraplegia in association with chronic valvular disease of the heart, ulcerative endocarditis, arterial degeneration, or venous stagnation of other parts. Whether continued bodily exertion or excessive muscular effort can produce myelitis is extremely doubtful. It is also very rare for acute paraplegia to follow an attack of fever; possibly when it occurs it is of septic origin. In far more numerous cases acute myelitis follows syphilis. We must, however, remember, first the great frequency of

syphilis in certain portions of the population; secondly, the numerous cases of acute paraplegia without evidence of syphilis; and thirdly, the difficulty of diagnosis. Experience makes us cautious in assuming that the not infrequent cases of acute paraplegia which recover, and those which die without a post-mortem examination, are all due to acute softening of the cord.

That cases of acute myelitis, or, to speak clinically, of acute idiopathic paraplegia, are more common in men than women, may perhaps be taken as corroborative of its ætiological relation,—continued wetting of the lower limbs to fatigue in walking and to syphilis.

With respect to the chronic forms of intrinsic paraplegia, the grey induration or sclerosis of the cord, its regional distribution is in striking contrast to the prevailing diffuse or disseminated character of acute softening. But the histological condition is the same whatever the locality, and therefore we cannot but surmise that some common immediate cause must produce the lesions which from their different seat give rise to such diverse clinical disorders as locomotor ataxia, spastic paraplegia, essential spinal, bulbar, and other forms of atrophic paralysis, to which we may add, as both anatomically and pathologically related, insular sclerosis and general paralysis of the insane. Although for convenience of description we are obliged to define the anatomical locality and the

clinical symptoms of these various diseases separately, yet both at the bedside and in the dead-house we constantly meet with cases in which both symptoms and lesions are combined—amyotrophic with spastic paralysis, tabes with motor paraplegia, ataxia with muscular tremors or spasms, and general paralysis supervening on tabes. We are therefore perhaps justified in expecting to find a common ætiology for these diseases.

Whether the expectation is just or no, it is certainly not fulfilled; and few ætiological statements are true of even a small group of chronic intrinsic lesions of the cord. From this point of view it would be well first to separate degeneration of the grey matter, which is commonly primary and sometimes acute and even febrile in its course, from chronic sclerosis of the several white columns, which is in most cases secondary and probably always slow, gradual, and insidious.

The former pathological group corresponds clinically with atrophic paralysis, those cases excepted which are due either to peripheral neuritis or to primary muscular atrophy. These affections of the anterior cornua have long been recognised as infantile palsy, Cruveilhier's paralysis, and bulbar paralysis. The frequency of the most acute variety of this disease in young children distinguishes it from all other forms of chronic spinal disease. Unlike some other nervous disorders of children, it appears to be equally common in boys and girls.

The observations of Sinkler in the United States, and of Ross, Gowers, and W. H. Barlow in England, concur in the result that cases are far more common in the five summer months of May to September inclusive, and particularly in July and August, than at other times of the year. In the latter two months Dr Sinkler recorded seventy-seven cases, compared to seventy-two in the ten other months; and Dr Barlow forty-eight, compared to sixty-three in the ten other months. These facts must be ætiologically significant, but what they signify is hard to say. I have only met with a single example of infantile palsy attacking two children in the same family. It is certainly not hereditary, and though it sometimes follows an exanthem, is more often preceded or accompanied by slight pyrexia, which seems to be part of the disorder. Negatively it may be asserted to have no relation to rickets or tubercle or syphilis, and its dependence on a chill is to say the least unproved. The extreme rapidity of the morbid process, which is usually complete in a few hours, suggests some vascular disturbance rather than inflammation.

Atrophic paralysis with a subacute or chronic course differs ætiologically from the last kind in being extremely rare in children, seldom found in young adults, and most frequent after thirty-five. Its ætiology is quite unknown, and accordingly it has been ascribed to injury, to cold, and to intemperance.

Another form of atrophic paralysis, also of spinal origin, but differing from the last-named group in its extreme chronicity and in its being long limited to certain groups of muscles, is that to which Cruveilhier's and Aran's original cases belong, and which Lockhart Clarke showed to be, at least in most instances, of nervous origin, the *amyotrophée progressive protopathique* of Charcot, poliomyelitis anterior longissima. This form, less rare than the preceding in young adults, is far more common in men than in women. Friedreich found the numbers seventy-six to thirty-three, and Sir William Roberts eighty-four to fifteen. The cause of this remarkable affection is also completely unknown, but on pathological as well as clinical grounds it must be separated from the other forms of paralysis and atrophy; as, for instance, amyotrophic lateral sclerosis, hypertrophic cervical pachymeningitis with secondary atrophy of the arms, and hydromyelus, syringomyelus, or multiple glioma of the cord, which may all produce atrophic palsy.

When primary destruction of the anterior motor cells affects the bulb instead of the cord, the clinical picture is very different, but pathologically progressive bulbar paralysis or glosso-labiopharyngeal palsy belongs to the same group as Cruveilhier's cases. It also is more common in women than men (thirty-four to nineteen among Kussmaul's cases), and is rare under middle age. Of its true ætiology we are completely ignorant.

The cases of progressive atrophic paralysis which occur in children do not all belong to the same pathological group. Duchenne's cases may perhaps be regarded as early instances of Cruveilhier's palsy. Those described by Erb as juvenile progressive atrophy of the muscles affect particularly the age of puberty, and are decidedly hereditary, but probably these cases may prove to be myopathic in origin and analogous to the pseudo-hypertrophic paralysis of children. The hereditary cases of atrophic paralysis collected by Dr Tooth, and particularly the peroneal group of cases to which he has drawn attention, may also probably have an origin in the motor nerves or in the muscles themselves, and if we group these with the late Dr Meryon's cases of muscular atrophy, we should be led to associating an hereditary character with a peripheral rather than a spinal origin.

The group of tabid symptoms more or less definitely associated with sclerosis of Burdach's columns are the most common, at least in this country, of chronic intrinsic affections of the cord; and, unlike lateral sclerosis, the lesion to which they are most often due is idiopathic. We ought, therefore, to be able to form some estimate of the natural history and origin of this remarkable disease; and if positive conclusions could here be reached, they would probably throw light upon the nature and origin of chronic indurating myelitis in general. We have, however, at present scarcely made more than

a beginning. Excluding all cases of more or less marked ataxic symptoms in which the characteristic anatomical change is not found after death, and mixed cases in which other regions of the cord beside Burdach's tract are involved, we should be inclined by analogy of its histological characters to compare this lesion with a chronic indurating form of inflammation in other organs, with cirrhosis of the liver, with chronic interstitial or fibrous inflammation of the lung, with the chronic atrophic form of Bright's disease, and with fibrous atrophy of the testes. But for these it is difficult to find a common cause. They all belong to the later period of life, and may be regarded as degenerative as well as inflammatory changes. Indeed, it has often been argued in one or the other case that they are not inflammatory, but purely degenerative. This contention is to my mind certainly disproved by the facts of histology, as well as by other considerations, in the case of the lung, the liver, and the kidney; and the weight of evidence is to the same effect in the case of the nervous centres. Beyond this very general accordance there is little that these processes have in common, in their apparent origin. Hepatic cirrhosis is, with rare and obscure exceptions, of direct alcoholic origin; the granular kidney is much less frequently so, and the other forms of disease which we have here grouped together are probably independent of this cause. The fibroid testis is almost always a syphilitic lesion, but syphilis of the liver is quite

distinct from true hepatic cirrhosis, and syphilis of the lung from true pulmonary cirrhosis; while in the case of the kidney the only change which is sometimes the result of syphilis is the lardaceous transformation. Tuberculous inflammation, again, is the origin of those cases of cirrhosis of both lungs which are known as fibroid phthisis, but there is no corresponding chronic tuberculous lesion of other organs except the fibrous induration of the adrenals which is met with in cases of morbus Addisoni. All, then, that we can draw from this analogy is that we should expect to find the origin of posterior sclerosis of the cord in long-continued but slight irritation, conveyed by the blood, and of a chemical rather than mechanical nature. We should expect the disease not to be hereditary, and not to be caused by changes of temperature or by excess of functional activity—if the latter is ever a cause of any disease. As a matter of observation our negative anticipations are realised. Tabes is certainly not hereditary, nor traumatic, nor alcoholic, nor tuberculous in origin. It is, as all observers agree, far more common in middle adult life than in youth or age, and of all nervous diseases is most decided in the predominance of male patients. Of eighty-three consecutive cases in Guy's Hospital, seventy-five occurred in men, and only eight in women.

The supposed causation of tabes by exposure and fatigue, and particularly by the hardships of a cam-

paign, may perhaps be sufficiently explained by remembering that it is only in adult males that tabes (whatever its cause) is common.

The relation of this remarkable disease to syphilis cannot be denied, although the explanation is very difficult. Fournier and Vulpian in France, Erb and most German neurologists, and Buzzard and Gowers in this country, all agree in stating that from half to three fourths of tabid patients have had syphilis.*

We must remember that the facts just mentioned of the incidence of tabes with regard to sex and age would explain its occurring in a great many patients who had suffered from syphilis. For instance, it is quite possible that more than half of the broken legs in hospital belong to persons who have suffered from some form of venereal disease, because the accident and the disease are both more common in young adult males than in other portions of the population. What to my mind is the strongest argument in favour of an ætiological relation of tabes to syphilis is that those cases where the presence of lues is certain are the ones that most improve under treatment. One patient of my own, with all the characteristic signs of tabes, and with scars of healed gummata, lost all his symptoms under a course of mercury; and when, nearly two years afterwards, the tabid symptoms returned, a similar

* 'Transactions of the International Medical Congress for 1881,' p. 32.

course of treatment was again followed by their disappearance. Another patient, the subject of equally marked tabes and preceding syphilis, has been under my observation for several years. He has completely lost his lightning-pains, walks securely, and in fact enjoys good health, but has more than once suffered from a return of his symptoms, which in each case is checked by a return to the remedy. In this patient it is remarkable that the lost knee-jerk has never been recovered.

It has been thought that the relation of tabes to syphilis is that of a sequela to a fever, because a certain proportion of cases of this disease, as of insular sclerosis, follow acute disorders. But this is very exceptional; the only instance of frequent and undoubted sequence of a nervous disorder on a fever—that of paralysis after diphtheria—is now ascertained to be not of central, but of peripheral origin.

Diseases of the Brain

The contrast between the diseases of the brain and spinal cord is one of the most striking surprises to an intelligent student. Closely allied in their development, their histology, and their functions, they yet differ widely in their pathology. The common idiopathic diseases of the cord—acute myelitis and chronic sclerosis, and paraplegia from compression—have scarcely any counterparts in the brain. Cerebral paralysis is in most cases due to

disease of the cerebral arteries, but spinal hæmorrhage is as rare as cerebral is common.

Phrenitis, or "encephalitis" or "congestion of the brain," as an idiopathic affection accompanied with delirium and other nervous symptoms, is a mere figment which ought to be banished from rational medicine. Cerebral abscess is always secondary, and if any disease deserves the name of acute inflammation of the brain it is the inflammation of the pia mater and subjacent grey substance, which is with few exceptions septic, tubercular, traumatic, or epidemic.

The ætiology, therefore, of nine tenths of cerebral diseases is really the ætiology of thrombosis and of embolism, of atheroma, of Bright's disease, of new growths, of syphilis, of tubercle, or of pyæmia.

There remains, however, one remarkable and purely nervous disease of the brain : it forms a link between disorders of the mind of which the anatomy eludes our search, and those of the tissues which can be demonstrated to the eye. The histology of general paralysis of the insane is closely related to that of tabes and other chronic idiopathic scleroses of the cord, and a clinical connection is shown by the cerebral affection often supervening in the course of spastic paraplegia, locomotor ataxia, or insular sclerosis. Like tabes, it is undoubtedly more common in men than in women ; like tabes, it is almost unknown in childhood and old age ; like tabes, it has been ascribed to syphilis, to sexual

excesses, to injuries, and to preceding acute diseases; lastly, like tabes, its true ætiology, its *causa causans*, remains unknown.

Functional Nervous Disorders

The ætiology of these neuroses is, in almost every instance, still to seek. Some of them may be described as exaggerations of individual eccentricities, of bad habits, involuntary gestures, and tricks of movement which have never been repressed by education. It is remarkable how often among the lower orders the natural beauty of a child's face is marred by its continual grimaces. An inexperienced speaker will betray his nervousness or enforce his conviction by twenty odd movements and ungainly gestures, which, if they became fixed and habitual, might fairly be called spasmodic neuroses. Some of them are emotional outbursts, others are consensual movements dependent upon the law of irradiation, by which a powerful motor stimulus is apt to overflow beyond its intended channels. To the first group belong the smiles, the tears, the shuddering, and the involuntary applause of the spectator of a drama. To the second, movements of the face, the hands, the arms, and the whole body which accompany the words of the actor. These, when properly restrained and disciplined, form half the power of the orator, but they have their origin in the uncouth movements of a clown at a fair,

or of an illiterate person painfully composing a letter.

These involuntary, emotional, or consensual movements may, under certain circumstances, be developed into paroxysmal neuroses—on the one hand into spasmodic tic, on the other into hysterical convulsions; and in accordance with this origin is the asserted fact that such functional disorders are more common in France and Italy than among the less emotional and less demonstrative Britons.

It is observable that emotional disturbance not only excites the reflex centres to tears or laughter, to violent gestures or cries of terror or passion, but also inhibits the reflex movements of breathing, and disturbs the normally unconscious action of the heart, and the almost involuntary mechanism of speech. The paroxysms of rage, like those of coitus and parturition, are due to loss of cerebral inhibition; *impotentia sui* denotes not power, but violence and weakness. They have their morbid counterpart in epileptic convulsions. But the breathless suspense of hope and fear, the disturbed respiration of sighs and sobs and groans, are, like stammering, the effects of inhibitory meddling, and have their pathological allies in emotional retention and incontinence of urine, emotional diarrhœa, blepharismus, and nystagmus.

Another kind of local spasmodic neuroses have a different ætiology. I refer to the group of nervous

disorders named by Dr Weir Mitchell "functional spasms," a phrase which does not refer to their having no anatomical basis, but to their following the too-prolonged exertion of some motor function.* Perhaps the phrase "fatigue-spasms" is more significant and convenient than any other. This kind of cramp or tonic spasm with weakness, which follows the over-use of muscles in a particular manœuvre, includes, beside scriveners' palsy and pianists' cramp, the lock-spasm of watchmakers, of telegraph workers, and of milkmaids, and other effects of a habitual handicraft. Not far off in their ætiology may probably be grouped saltatorial spasms, eclampsia nutans, malleation, and spasmus nictitans.

A distinct ætiological group of spasmodic neuroses, almost confined to children, and more common in boys than in girls, is closely related to rickets. Tetany or tetanilla, carpopedal contractions, trismus neonatorum, and laryngismus stridulus—these may all be regarded as exaggerated and perverted reflex-spasms occurring when the higher centres have not attained, or have lost, their inhibiting action over the spinal system. Their relation to one another and to rickets has been well brought out by Dr Cheadle, and a remarkable association of the latter disease with infantile convulsions and zonular cataract is one of the contributions to the true

* Dr Gowers calls them "occupation-neuroses," and in Germany they bear the designation "Beschäftigungsneurosen;" translated, without much sense of humour, into "professional spasms."

philosophy of disease which we owe to Mr Hutchinson.

Another remarkable spasmodic nervous disorder, is that described by Dr Thomsen in 1876 as "Tonic contractions in voluntary muscles, the result of inherited psychical predisposition"—*myotonia congenita*,—and since generally known by his name. It has relations in more than one direction; first with reflex neuroses like saltatorial and salaam-spasms, secondly with stammering and allied emotional spasms, and thirdly with hysterical convulsions. Its peculiar ætiological note is its remarkable distribution in families.

Like Friedreich's disease, it is not always hereditary, but occurs in brothers or sisters without being transmitted from or to the next generation. This lateral, rather than vertical, distribution may be sometimes observed in rheumatism, in insanity, and other diseases of more or less hereditary character. It seems to point to an ætiological condition which is not directly derived from either parent, but is the result of two strains combining.

Of the most serious of all functional neuroses, Epilepsy, we must admit that its exact pathology is still undetermined; its ætiology is therefore also uncertain. Our first difficulty is that of separating cases of symptomatic eclampsia due to cerebral tumours, to injury of the head, to uræmia, puerperal convulsions, infantile convulsions, and poisoning, especially by lead. If such cases, of

which the ætiology is recognised, be separated, the cases of primary idiopathic epilepsy form a more natural and manageable group. They agree in the early period of life at which they most commonly begin, in their recurrent and ingravescent character, in their sequelæ, and, we must add, in their bad prognosis. Thus limited, there is no doubt that hereditary predisposition would rank in the first place amongst the causes of this terrible disease. Next to it must be recognised the convulsions of teething in infants, and alcoholic and sexual excesses in adults.

Lastly comes that remarkable neurosis described by Sydenham, and hence known abroad, as *Chorea anglorum;* and also as chorea minor, to distinguish it from the chorea Sancti Viti major, the dancing mania of the Middle Ages.

Dr Sturges has well insisted upon the relation of chorea to the fidgetiness and face-pulling and ugly tricks to which I have already referred as the physiological type of certain neuroses. If a healthy child could not fidget and grimace, the symptoms of chorea would not be what they are. This I fully admit; but it only means that the symptoms of disease have a physiological basis, and has, I think, no decisive bearing on the ætiology of chorea. A soldier does not know he is wounded until the excitement of the action is over; a mother does not feel her burnt hand until her child is saved from the fire; and if temporary anæsthesia was not thus

producible in healthy persons under strong emotional excitement, we should not see the hemianæsthesia of hysteria. If a child did not laugh and cry, and struggle when tickled, it would not have the mechanism by which infantile convulsions, and even adult epilepsy, is produced. But though a cough is a physiological reflex action, bronchitis is more than frequent and violent coughing.

Morbid agents can only act upon the pre-existing physiological machine. All pathological states, fever, delirium, dropsy, inflammation, are but perverted working of the healthy organism. But although, to return to the example before us, the grimaces of chorea are those of a restless child,* and the most violent movement of its limbs is effected by the same nervous and muscular mechanism which produces the corresponding voluntary gestures, yet we do not, I think, find that this disease is more frequent in fidgety children than in others; while the usually definite and often almost sudden onset, the pathological associations and the clinical course of chorea, stamp it as a definite disease.

The most important points in its ætiology are: first, its marked predilection for childhood, or the period between the completion of dentition and the

* There is, however, a difference which would enable one to distinguish the grimaces of chorea, if represented by a skilful painter. In the choreic child the eyes do not show the emotion which is simulated by the mouth; they have a blank impassive expression, which makes the facial movements seem like those of an inanimate puppet moved by strings.

establishment of puberty. Secondly, its greater prevalence in girls than in boys, and the still more decided prevalence of adult chorea among women.* One occasionally sees a delicate ill-developed youth, who, having been choreic before puberty, will continue subject to the disease up to twenty or twenty-five; and I once saw genuine chorea with rheumatic and cardiac complications in a man as old as forty. But while such cases are rare exceptions, chorea is not uncommon in girls up to the age of eighteen or twenty, and has long been recognised as either reappearing or appearing for the first time in the pregnant or puerperal state. The cases of senile chorea which have been from time to time described,† belong, I venture to think, to a distinct pathological group.

Next to its well-marked relations to sex and age, chorea is undoubtedly associated with rheumatism, using that word as a synonym for rheumatic fever. This connection was first pointed out on the Continent by Professor Sée, but had long previously been recognised in England by Dr Copeland, by

* In three series of patients from Guy's Hospital I found that there were 423 females to 159 males. At the same hospital the late Dr Hughes, in 1846, reported 240 females to 69 males. Similar results have been obtained by Professor Sée in Paris, by the late Dr Hillier at Great Ormond Street, and by Dr Goodhart at the Evelina Hospital. The largest proportion of boys to girls that I have met with was that found by Winker at Leipsic in 1844, 117 to 210.

† Particularly by Charcot; previously by Graves, and subsequently by Dr Saundby.

Hughes, by Bright, and by the elder Babington, who stated in his lectures so far back as 1811 that chorea often followed rheumatism, and that these two diseases often come on alternately. (See Appendix.)

The connection between the two diseases does not only rest on this frequent clinical association, but also on the anatomical fact of endocarditis being almost always present in fatal cases, since valvular disease in children is in the great majority of cases definitely connected with rheumatism. Lastly, chorea resembles rheumatism in the age of the patients it attacks, and in the frequency of its recurrence.

It is more difficult to determine the nature than the fact of the connection. The evidence seems to be in favour of regarding rheumatism as a predisposing cause to chorea, not *vice versâ*, and the cardiac murmurs, the endocarditis, and the occasional pericarditis of chorea as due to rheumatic inflammation. That in children the joints are so little affected and the pyrexia so slight may well account for a history of rheumatism being often absent. I have seen, not its previous occurrence, but its present existence, unsuspected by the patient's mother.

Ætiology of Cardiac Diseases

Pericarditis.—If we compare the affections of the three divisions into which the great somatic cavity is separated, we find, as we should expect, a general

agreement in their pathology. This agreement extends, though to a less degree, to the so-called arachnoid space, the synovial cavities of joints, the air-vesicles of the lungs, and the areolæ of connective tissue generally; for these spaces also, though never parts of the cœloma, are formed by cavities in the mesoblast, are lined with endothelium, and are bathed in lymph.

Notwithstanding, however, the common liability of pleura, pericardium, and peritoneum to a characteristic serous or fibrinous or purulent form of inflammation, and to secondary multiple tubercle and carcinoma, we find important ætiological differences between them, which we can only describe as topographical; that is to say, not referable to histology or development or obvious local environment. Thus the pleura is very prone to idiopathic inflammation, more or less doubtfully ascribed to cold, and this, except in children, is very rarely purulent. The pericardium and peritoneum are almost incapable of a similar acute idiopathic inflammation. The pericardium is highly susceptible of rheumatic inflammation, the peritoneum insusceptible, while the pleura is in this respect intermediate. The pleura and the pericardium, like the connective tissue of the lungs and the subcutaneous fascia, are extremely liable to inflammatory dropsy or œdema in the course of Bright's disease, while the peritoneum is scarcely ever so affected, unless stimulated thereto, as by paracentesis.

The causes of pericarditis which remain after we have accounted for those due to rheumatism (in children and young adults) and to Bright's disease (chiefly in older patients) are comparatively few, and of pathological rather than clinical interest.

Pericarditis is occasionally septic, arising either from general pyæmia or from local infection by an abscess or empyema. Pyæmic pericarditis is, in Dr. Wilks's experience, always due to rupture of a secondary cardiac abscess.

It is generally stated that pleurisy may spread to the pericardium; but the instances are certainly rare, and will probably become more so on careful scrutiny, for many would turn out to be cases of rheumatism with but slight articular affection, and others tuberculous, while others again are not related to simple pleurisy but to pleuro-pneumonia. We cannot speak, except metaphorically (and therefore obscurely) of a process like inflammation "spreading" from one organ to another. Bacteria or leucocytes may migrate, infective liquids may pass through lymphatics or blood-vessels; but unless we can demonstrate the presence of the products of inflammation in the tissues between the pleura and pericardium, we cannot properly say that the inflammation has spread—as it does, for instance, in a case of eczema or of a boil, in pneumonia or phthisis, in dysentery or diphtheria. Either there has been conveyance of something material from the one serous sac to

the other, or we must explain the occurrence as a similar independent affection of the two membranes closely resembling each other in structure, function, and origin. Indeed, when pleurisy appears first on one side and then on the other, it appears the truer view to regard the two pleuræ as a double organ like the kidneys, the adrenals, or the lungs. Thus we may say that whatever are the conditions which cause inflammation of the right pleura, they are more likely to produce the same condition of inflammation on the left than on any other part of the body, that the part next most likely to be affected is the pericardium, and after that the peritoneum. The fact that pericarditis when due to tuberculous or cancerous nodules is often associated with a similar condition of both pleura and pericardium, may be compared with those instructive cases in which without the presence of tubercle and cancer, all the serous membranes undergo simultaneous inflammation.*

Idiopathic pericarditis is as rare as idiopathic pleurisy is common; nevertheless such cases occur. I saw one several years ago, and another befell a distinguished Fellow of this College, Professor Bäumler,

* In a case which I watched during several years, the patient, who died when above sixteen years old, suffered from recurrent attacks of subacute pleurisy, pericarditis, and peritonitis with hydrocele. At the post-mortem examination the body was well grown and well nourished, but all the serous membranes were greatly thickened with serous effusion into the thin divisions of the pleuro-peritoneal space, but there was no affection of the viscera, and no trace of tuberculous or other primary disease.

of Freibourg, who, interested in his own case, afterwards published a monograph on the subject.

Valvular disease.—Inflammation of the endocardium is probably never idiopathic; one can scarcely imagine the most profound chill reaching to the heart. The true cause of ulcerative endocarditis is still unknown. The micrococci first described by Heiberg, of Christiania, are not constantly present, although their place is taken by other forms of infective microbes—staphylococci, streptococci, and bacilli. This formidable disease is almost always secondary to a valvular lesion from previous endocarditis, and, as Dr. Osler showed in his interesting lectures before this College, frequently follows an attack of lobar pneumonia. It also occurs occasionally during scarlatina, diphtheria, and puerperal fever. But it does not appear to develop in the pyrexia of rheumatism itself, and is certainly not common in surgical pyæmia. I have seen it occur after pneumonia and during enteric fever, but more often without any assignable cause in a case of previous rheumatic endocarditis.

Simple acute or subacute inflammation of the lining membrane of the heart is seldom, or perhaps never, a consequence of Bright's disease in any of its forms, and it is probable that with scarcely an exception this grave lesion has its origin in rheumatic fever, including the slighter cases which precede chorea in children, and those which follow scarlatina. It is certainly never produced by

osteo-arthritis, by gout, or by gonorrhœal synovitis.

Congenital valvular lesions are for the most part due to intra-uterine endocarditis, probably rheumatic in origin. Those due to arrest of development are much fewer in number, because the subject of such malformation when severe dies at or soon after birth.

The ætiology of the chronic valvular sclerosis which comes on in later life and is independent of rheumatism is somewhat obscure. Where there is chronic arteritis with calcareous degeneration which has spread to the aortic valves, or where similar atheromatous changes are found with a contracted mitral orifice, the process may be certainly referred to the results of strain. The wear and tear of the circulation produces loss of elastricity, chronic inflammation, and degeneration of the arterial coats. This naturally begins about thirty or sometimes earlier still; it increases with advancing years—we may almost say with the increasing tale of the pulse-beats; it is most advanced in the parts of the arterial system most exposed to strain, and it appears earliest in those persons whose vascular system is most severely exercised. Thus we see the first appearance of sclerosis at the weak point of the aorta, its arch, and particularly at the junction of the middle and descending portions, the origin of the innominate artery and the sinuses of Valsalva. It chooses, beside the aortic arch, the popliteal artery,

which is stretched with every vigorous extension of the leg, the cerebral arteries, and particularly the cerebral artery which supplies the motor tract, and the renal arteries where the chronic contracting form of Bright's disease has caused high pressure in the vessels which supply the cortex. When present in the pulmonary artery we find evidence of chronic emphysema and dilatation. Apart from old age, atheroma belongs by preference to the male sex and to the laborious classes. When it occurs in a woman we usually find that she is of powerful frame and has followed a laborious occupation. When it occurs under sixty we usually find that the patient has been a soldier or a sailor, a porter or waterside labourer. The disease is not produced by "overwork" in the proper sense of the word, any more than by idleness; it is produced by direct strain on the arteries, either longitudinal stretching of their coats or the internal tension of high blood-pressure. Its relation to gout is, I think, only indirect, for chronic arteritis has none of the clinical, anatomical, or chemical characters of true gouty inflammation; but it has the same range as gout, affecting men much more than women, and the later much more than the earlier decades; and it is produced, or at least favoured, by the high pressure of chronic renal disease which so often accompanies gout.

When we find disease of the sigmoid valves which is not due to atheroma, it is almost always the result

of rheumatic fever; when we find aneurysm which is not due to atheroma or to local injury, it is almost always the result of syphilis.

There remain, however, not infrequent cases of chronic sclerosis of a valve which is without atheroma, either there or in the aorta, which affects women more often than men, and the mitral more often than the sigmoid valves. Such stenosis of the mitral or of the mitral and tricuspid orifices without rheumatic antecedents, has sometimes been explained as the result of chronic renal disease and sometimes as the effect of repeated pregnancies, but its ætiology is certainly far from clear.

The origin of *arterial atheroma* or chronic arteritis deformans may be gathered from its natural history. Its principal seat is in the large arteries, spreading, we may say, from the arch of the aorta; it seldom reaches to branches smaller than the renal and cerebral vessels; and in the other direction, while it constantly encroaches on the aortic (sigmoid) valves, it is comparatively rare in the mitral, still more rare on the right side of the heart and in the non-valvular endocardium, and is only found in the pulmonary artery in cases of long-continued obstruction in the lesser circulation.

This remarkably constant distribution points to an association of atheroma with strain and tension of the arterial walls, and the same conclusion is reached by other facts in its natural history. One of these is its habitual occurrence after the legitimate

wear and tear of fifty or sixty years; others are its association when prematurely present with particularly laborious occupations, its rarity in women, and its occurrence in these rare cases as the result of labour more severe than is usually borne by the weaker sex. The evidence that atheroma is due to such mechanical cause appears conclusive.

On the other hand, there seems to be no reason to connect it specially with intemperance or gout, or rheumatism or Bright's disease or lead poisoning. The coincidence may in each case be explained by the two diseases being common in the male sex, and in the later periods of life. The chronic arteritis due to syphilis is probably distinct in its nature, as well as its origin, from the far more common atheroma.

In the production of aneurysms, both aortic and peripheral, the influence of strain has been long recognised, but it appears rarely, if ever, to act directly. The first effect is weakening of the arterial tissues by atheromatous inflammation, and then the damaged vessel yields to the pressure of the blood.

Aneurysms occurring in young adults, and unexplained by any exceptional mechanical strain, are almost always due to syphilis.

The pathology of pulmonary aneurysms in phthisis is a totally different one, and that of miliary aneurysms of the cerebral arteries is still unknown.

Ætiology of Diseases of the Lung

A sudden chill, or imprudent exposure, a draught of wind or a shower of rain, wet feet, or sleeping in damp sheets, furnish the ready explanation of sore throats and coryzas, laryngitis and bronchitis, pleurisy, pneumonia, and consumption.

That these supposed causes do not produce all these effects alike, and that their influence is seldom direct and uncomplicated, we must admit. But I cannot agree with a respected Fellow of this College in denying their influence altogether ;* again and again many of us have experienced in our own persons the sudden chill which produces sneezing and coryza, or the impression of a cold east wind upon the face or hands which has the same effect; when wet through we must have felt the throat becoming sore, and the voice hoarse; and in many cases we have also found that the remedy for "a *cold*" is to keep *warm*, that a change of clothes, a hot bath, or a few hours in bed, will cut short the incipient attack as no specific fever can be cut short.

The distribution of catarrhs, bronchitis, and pleurisy, both as to seasons and to climate, is further evidence to the same effect.

One practical point in the ætiology of angina, throat-deafness, and bronchial catarrh is that when the Schneiderian membrane is swollen by inflam-

* See an able address by W. H. Ransom, M.D., F.R.S., on "Cold as a Cause of Disease," 1888.

mation, the passage of air through the nostrils is obstructed, and the patient is compelled to breathe through his mouth. In fact we see exactly the same results when the nasal passages are obstructed by lymphatic overgrowth in the pharynx and posterior nares. The result is that the inhaled air, unwarmed and unmoistened by the passage through the nasal fossæ, irritates first the fauces, and next the trachea and bronchi. Its coldness and dryness check secretion, and produce the characteristic tickling which precedes angina and bronchitis; while the inorganic and organic particles in suspension are not entangled among the vibrissa or caught on the mucous lining of the nostrils, but gain an entrance to the crypts of the tonsils or the stomata of the pulmonary air-sacs.

When, however, we have admitted that many affections of the respiratory organs are directly caused by cold,—that a cold in the head is usually the effect of a draught or of exposure to an east wind, that hoarseness is often the result of talking in the open air, bronchitis of breathing cold air through the mouth, and pleurisy of getting wet through,—we have only accounted for a part of the diseases in question. Bronchial catarrh may undoubtedly be produced, as in fever, by accumulation of mucus in the smaller bronchi. Some feverish colds are as contagious as epidemic influenza, and acute pleurisy may come on in a patient warm in bed, if he has diseased kidneys. Or, to look at the

question from another point of view, what is it that determines whether a man who has sat in wet clothes will have pleurisy, rheumatic fever, lobar pneumonia, nephritis, or myelitis, or whether he will escape altogether? We may see each of these affections occasionally attack a patient who is confined to the house, or even to his bed, by some chronic malady or even by a broken leg. The epidemic and contagious character of influenza, and the occasional appearance of lobar pneumonia in a group of apparently causally related cases, together with the impossibility of drawing a clinical or pathological distinction between such cases and ordinary sporadic pneumonia or feverish cold, prove that we must not assume, because some cases are due to an ascertained cause, that all similar cases are due to the same; nor that when a definite microbe is constantly associated with a particular form of disease, that we have solved the question of its ætiology.

Even the great question of the origin of phthisis has not been fully answered by Koch's brilliant discovery of the bacillus of tubercle. That this is a definite recognisable microphyte is certain; and certain also that it is constantly present in lesions which are histologically tuberculous; and in particular in all forms of chronic caseous inflammation of both lungs, beginning at the apex, travelling downwards, and marked by the presence of lobular pneumonia, ulceration, bronchitis, pleurisy, and cica-

trisation, with the presence of grey and yellow granulations. In other words, all pulmonary phthisis and almost all chronic disease of both lungs is tuberculous, infective, and specific. This has, I take it, been conclusively proved. "Without the specific microbe, no phthisis"—that we admit, but how many other conditions beside its presence are necessary to produce the disease? The importance of hereditary predisposition is surely put beyond question not only by our daily experience in practice, but by the records of life assurance. The local effects of pulmonary catarrh in preparing the soil for the reception of the invader is proved by the undoubted existence of a large pathological group of cases caused by the irritation of solid mineral particles. Tubercle bacilli are found in the lungs of grinders and potters, but no one is so foolish as to throw away previously acquired experience, and deny that such trades cause consumption. They cause consumption (we must now believe) by producing pulmonary catarrh, and thus facilitating the entrance, survival, multiplication, and successful establishment of the parasitic plant which causes the anatomical lesions of phthisis. Moreover, we learn from the almost universal prevalence of consumption in certain trades that the seeds of the disease must be constantly present in the workshops, say, of Sheffield grinders, probably more common than in hospital wards or in ordinary dwelling-houses—so that these places are (not rhetorically,

but literally, "hotbeds of disease." The sputum which one reckless, high-paid, short-lived, consumptive workman ejects from his ulcerated lungs dries and becomes dust, which is carried into the air-passages of his neighbour, whose lungs have already begun to suffer from the particles of silica which he inhales. First he is wounded, next the wound is poisoned. Passing through the stomata of the air-vesicles into the subjacent lymph-spaces, the virus is conveyed to the lymph-glands, and then more gradually to distant organs.

So far seems clear, but many interesting ætiological problems await solution. Why are young adults most subject to this disease? Are they more exposed to the poison than old people; or more liable to the precedent catarrh; or more apt to become anæmic; or is the lymphatic absorption in them more active?

Is catarrhal lobular pneumonia, particularly that which follows measles and pneumoconiosis, the only local preparation for the seed? Why is not bronchitis? Why is not lobar fibrinous pneumonia? Do the bacilli themselves ever act as an irritant as well as an infective agent?

Apart from the lungs, why does not the virus more often gain access through the digestive mucous membrane? Is it ever admitted by a wound? How does it gain access to joints and bones?

Does the infection always take the common route

from the air-vesicles or lymph-follicles of the intestine to bronchial or mesenteric lymph-glands, and thence to the veins by the thoracic duct; or does it ever pass directly into the veins?—for instance, in cases of rapidly generalised tuberculosis of children, with but slight caseation (or none at all) of lungs or lymph-glands.

Pulmonary hæmorrhage has again and again been assumed to be a local "cause of phthisis," and numberless cases to prove the point have been recorded (cf. p. 32, *foot-note*).

This ancient doctrine, formally stated by Morton in the seventeenth century and by Cullen in the eighteenth century, was revived in our own time by Prof. v. Niemeyer, and made part of his now exploded theory of phthisis. By the light of our present knowledge, may we not say that a healthy lung never, or very rarely, bleeds under any strain, and that if it should, such primary pulmonary hæmorrhage is innocuous, or at least never produces phthisis? Hæmoptysis is not uncommon in cardiac disease, particularly in cases of aortic and obstructive mitral lesions, and yet phthisis does not follow it; traumatic hæmoptysis, as by a fractured rib, does not go on to phthisis; and injected into the trachea of dogs does not set up caseous changes in the lungs.

May we not then assert, in harmony with clinical experience, that hæmoptysis (without disease of the heart and aorta, without purpura or injury) though

often the first symptom, is never the first *event* in a case of phthisis?

The classical researches of the late Dr Bowditch, of Massachusetts, and of Sir George Buchanan in this country, have left no doubt of the association of phthisis with damp and undrained soil; but does this depend on the frequency of pulmonary catarrh under such conditions, or on a general depression of the resisting, protective, or, if you please, phagocytic property of the tissues?

Do starvation, anæmia, and other depressing influences act by diminishing the resistance of the patient, or by favouring a preliminary inflammation of the bronchioles? The same question applies to hereditary predisposition to the disease, and to the condition of dyspepsia, anæmia, and loss of weight and strength, which so often precede pulmonary symptoms.

Happily, even the briefest discussion of the ætiology of phthisis would be incomplete without at least a mention of the antagonistic as well as the favouring conditions of the disease.

And first, of age. Though senile phthisis is not absolutely rare, it is relatively so. Most cases that I have seen have been very chronic, and sometimes relapses after recovery from consumption in youth. A first successful invasion of the infection is certainly very rare after fifty. Again, phthisis in children is comparatively rare, and congenital phthisis is almost without example.

Free ventilation is certainly inimical to the lodgment of tubercle; no doubt partly by diluting the contagium, and partly by keeping up the appetite and power of resistance of the host.

A very cold and a very hot climate seem both antagonistic to phthisis; but, while the former seems to be in some measure curative as well as prophylactic, the latter is rather injurious than beneficial when the malady is once established. Cold probably acts directly on the bacillus, which does not grow below a certain temperature, and heat perhaps by diminishing the predisposition offered by pulmonary catarrh.

Carnivorous animals are much less liable to tubercle than herbivorous, but the rule is liable to exceptions, as in the case of goats; and there is no reason to suppose that among human beings, vegetable feeders are less exposed to phthisis than those who use much animal food.

Finally, even in diseases themselves we sometimes find allies.

Pulmonary emphysema appears to render the development of tubercle in the lungs more difficult; but rather by diminishing the vascularity and the amount of connective tissue in the lung and obstructing lymphatic channels than by its effects on the general circulation. Indeed, it seems to be well established that, so far from permanent cyanosis (and particularly that caused by congenital disease of the heart) being antagonistic to phthisis (as

Rokitansky supposed), the subjects of congenital pulmonary ("dextro-sigmoid") stenosis are particularly liable to consumption, if they survive puberty.

But cyanosis from disease of the right side of the heart with pulmonary anæmia is one thing, congestion of the lungs from disease of the left side of the heart is another. And I believe that mitral disease is really a negative factor in the ætiology of phthisis. I have myself had two or three cases in which the destructive lesions in the lungs were apparently checked by cardiac disease. There is a beautiful drawing by my late colleague, Dr Moxon, which he made to illustrate the same doctrine. Probably tubercular ulceration does not flourish in these cases because the chronic passive congestion favours fibrous degeneration—the cicatricial process to which we look to limit and starve the more dangerous and rapid disease.

It has been asserted that gout is prophylactic against phthisis and phthisis against gout, but this is probably a mistake; that the two may occur together most of us have probably seen; I have observed the conjunction more than once. That they do not often coincide is no more than what is to be expected from their incidence on different periods of life.

A similar criticism will apply to the presumed opposition between tubercular and malignant disease. Carcinoma affects elderly persons for the

most part, while the majority of phthisical patients are young adults. Children with tubercle die before they can develop sarcoma, children with sarcoma die before they can develop tubercle, and adults affected with tubercle too often die before the age at which cancer is most frequent. In a patient of my own who lately died of cancer of the stomach, we found both old and recent tubercle in the lungs.

Ætiology of Gastric Diseases

With respect to the most common disturbances of the stomach, it is difficult to speak with precision of their ætiology, because their pathology is still so obscure.

Arguing, however, from the known to the unknown, there is no doubt that one cause of dyspepsia is chronic gastritis from irritants, for in the case of arsenic and other irritant poisons, pain, nausea, vomiting, and a sense of heat and weight referred to the epigastrium are well ascertained to be due to catarrhal inflammation, hæmorrhage, or ulceration of the gastric mucous membrane.

In a less degree we can recognise the same results following irritating articles of food and alcoholic drinks.

Another and still more common source of irritation is furnished by food which is too hastily swallowed, unmasticated, and insufficiently insalivated. This acts as a foreign body, and checks in-

stead of exciting secretion of gastric juice; mucus is thrown out instead, and efforts of vomiting ensue.

The same result is probable produced by over-feeding. The excess of food remains undigested, bradypepsia becomes apepsia, and the residue becomes a source of irritation.

A second ætiological group of gastric disorders depends upon passive congestion either from primary or secondary disease of the heart, or as the immediate result of portal obstruction. Here we find the mucosa swollen, dark red, hard, and covered with tenacious mucus.

In many cases of dyspepsia, however, neither passive venous congestion, nor intemperance, nor hard or other indigestible food, nor hasty meals, nor over-eating, can be assigned as causes; and yet it is almost certain, from the time these cases last, from their sudden and often spontaneous cure, and from their capricious variations, that the disorder is functional.

Looking to the results of the laboratory, it is difficult to suppose that there is a deficiency of pepsin, but that the gastric secretion is inhibited or deficient in acid is probable enough. This again may be due to direct nervous inhibition of the secreting epithelium or to vaso-motor disturbance causing temporary anæmia. The cases correspond to what has been described as "atonic dyspepsia." But it is more to the purpose to recognise their nervous origin. A man who is reading letters and

giving orders while eating his lunch, a shopkeeper interrupted in his dinner, a mother carving for and keeping in order a family of children at their mid-day meal, a traveller swallowing a heavy meal with his eye on his watch—all these are the conditions of neurotic dyspepsia so common in early adult life.

Lastly, we may suppose, and the supposition is confirmed by positive facts, that the muscular coats of the stomach sometimes cease their natural movements, become relaxed and yielding, owing to disturbance of the nervous supply to their muscular coat. As soon as the normal pressure of the gastric walls on their contents is impaired, the nitrogen, carbonic acid gas, or hydrogen-compounds present begin to expand. This is probably the condition in cases of flatulent dyspepsia.

That the secretion of gastric juice and the movements of the stomach may be checked by direct cerebral inhibition seems certain from cases in which sudden anxiety or emotion checks at once both appetite and digestion, so that the food remains as a crude mass in the stomach until it is at last rejected. Minor degrees of the same influence are probably often present in the dyspepsia of early adult life. The rarity of such cases in children, before the distractions of life have begun, and in the aged after they have in a great measure ceased, is further evidence of their causation.

Of the true cause which leads to the frequent,

painful, and often fatal disease which is known as the round or perforating gastric ulcer of Abercrombie, Rokitansky, and Cruveilhier, we still know little. Many authorities, including Virchow and the late Sir William Gull, have taught that it is due to thrombosis of a gastric vein, and consequent action of the secretion of the stomach on its own walls. That this, however, is not the whole explanation seems clear, since in cases of mitral disease and cirrhosis of the liver there is often stagnation and occasionally clotting in the gastric veins; but in neither condition do we meet with the characteristic ulcer; nor, again, do we find its characters when corrosive poison or cancer has destroyed the mucous membrane and laid it bare to the action of the digestive secretion. The relation of ulceration of the stomach to chlorosis is generally admitted; but I would again draw attention to the fact that, with the exception of childhood, there is no age exempt from gastric ulcer; and that it is far from uncommon in men and in elderly persons of both sexes. (See Tables in Appendix.)

Ætiology of Hepatic Disorders

Cirrhosis of the liver is a disease of which the ætiology is well ascertained. Dr Payne has discovered an unmistakable figure* of a hobnail liver in the

* The figure was drawn and engraved by Faithorn; the case was described by John Browne, surgeon to St Thomas's Hospital, and author of the curious anatomical plates published in 1684, under the title 'Myographia Nova.'

'Philosophical Transactions' for the year 1685. It was found in the body of a soldier of the Guards, who died (it was charitably supposed) from drinking too much water ('Path. Trans.,' 1889, p. 314). Spirit drinking is undoubtedly the cause of the common hobnailed liver, and we may believe that the lesion was rare before distilled spirits came into use in the seventeenth century. It certainly may be caused by strong wine, but much less readily; and malt liquor appears to have still less effect. Whether this depends on the greater diuretic action of the latter drink is not clear; but it is certain that the anatomical effects of different forms of alcohol differ independently of the amount taken; as much so as the physiological derangements caused by brandy, by ale, and by the several kinds of wine. I do not know whether, as is asserted, London-made cirrhosis can be distinguished from a hobnailed liver produced in Glasgow; or whether in the West Indies rum produces as distinct a local variety as gin and whisky here. But while alcohol, in whatever form, has well-defined general effects in proportion to its amount and to the resistance of the patient, it is no less certain that the proclivity to one or the other morbid change is greatly dependent on the form in which the alcohol is taken. Cirrhosis, like delirium tremens, though almost certainly produced by a course of spirit drinking, is not apparently the result of intemperance in Burton ale or porter, which cause gout and obesity; nor of

the lighter German beer, which causes obesity but not gout.

Fatty degeneration of the liver often accompanies cirrhosis, and probably points to indulgence in malt liquors as well as in distilled spirit. As the effect of beer-drinking we see it associated with fatty overgrowth of the heart and muscles, and with general obesity. Intra-muscular fatty degeneration of the heart is not, I think, an effect of intemperance.

There are, however, certain cases of cirrhosis which cannot be ascribed to the abuse of alcohol. Some should be entirely separated, as those which depend on interstitial hepatitis with gummata, and the rarer forms without clinical importance which result from long-continued obstruction of the hepatic vessels, as in chronic valvular disease, or from similar prolonged obstruction of the bile-passages.

Others, forming the group called hypertrophic cirrhosis by Charcot, are supposed to be biliary in origin, and to have no relation to alcohol. But I venture to doubt this.

The not very infrequent occurrence of cirrhosis of the liver in children raises a curious question in ætiology. Once or twice I have seen this condition, with its characteristic clinical symptoms, explained by the presence of tubercles in the liver itself and elsewhere, but in most cases no such explanation is possible. Often, however, inquiry shows that the patient has been allowed to drink spirits. In other

cases, where no such history is forthcoming, it must be remembered that mere infants are sometimes dosed with gin as a narcotic, and one may well believe that, as a very little liquor will intoxicate a child, so a very little may suffice to start a process of interstitial hepatitis at an early age.

With respect to the ætiology of *jaundice*, it is remarkable how little has been accomplished since morbid anatomy and animal chemistry began to be studied. It is more than a century since the foundation of pathology as a science of facts was laid by Morgagni, and the knowledge of the chemistry of bile began with Berzelius; yet Boerhaave and Mead were not more ignorant than we of the cause of the most characteristic and remarkable form of jaundice. They, like us, could understand how when the duct of a gland is blocked by a calculus the secretion accumulates, distends the channels in which it runs, and is thus mechanically detained in the body.* But we, like them, cannot explain jaundice without obstruction.

Even in the case of obstructive jaundice much remains to be learnt as to the nature of cholæmia. If the bile, or any of its constituents, is so poisonous, why does it circulate for weeks and months without producing its effects? If it is innocent, why are those effects produced by acute yellow atrophy and

* Dr Vaughan Harley has shown that in the case of dogs, and presumably in that of man, the bile is reabsorbed not through the hepatic veins, but by the lymphatics.

by long-standing obstruction? Why is a manufactured excretion reabsorbed when the factory is reduced to ruins and the exit unimpeded; and why, in this case alone, are the symptoms of cholæmia as constant and speedy as in other cases they are tardy and uncertain?

But it is the cases of jaundice which we call simple that are most puzzling in their ætiology. We surely cannot be satisfied to refer icterus simplex to obstruction by the inspissated mucus, which no one ever saw, or by swelling of the duct or of the duodenum, of which there is no proof, or by the pressure of impacted scybala, of which the presence is entirely hypothetical. Do we find anything analogous in the case of the kidney, the pancreas, the mamma, the parotid, or the lachrymal gland? In many cases of jaundice is there the slightest evidence of duodenal catarrh, or of preceding obstipation? On the other hand, do we not frequently see evidence at the bedside, and proof after death, of considerable catarrh of the duodenal mucous membrane, and sometimes of fæcal accumulation in the transverse colon, and yet no jaundice? I might add the youth and health which usually marks the patient, and ask why the supposed cause of obstruction should, after a limited duration, disappear as mysteriously as it came.

Nor do we gain any help in explaining the cause of simple jaundice from Frerich's hypothesis of virtual obstruction owing to blood-pressure being

extremely low. For, in the first place, there is not the slightest evidence that in these cases blood-pressure is lower than usual. In the second place, we know that the pressure in the duct of a gland is often higher than that in the blood-vessels which supply it, without secretion being suspended or reabsorption taking place; and lastly, when we do know that the blood-pressure is abnormally low, as in prolonged syncope, in anæmia, and in distension of the right side of the heart from pulmonary obstruction, we do not meet with jaundice. The only case in which it does coincide with low pressure in the portal vein and high pressure in the hepatic is that of long-continued organic disease of the heart, and here we find evidence of mechanical pressure on the biliary ducts. Surely it is far better, instead of accepting vague hypotheses, which it is impossible either to prove or disprove, to regard jaundice in the absence of demonstrable obstruction as due to some yet unknown condition. We shall thus at least regard each case we meet with in daily practice as a problem to be solved, not as an example of a well-understood physiological process.

The fact is, that, in spite of recent advances in our knowledge of secretion, among which I may mention those due to our distinguished Bailey Medallist, Professor Heidenhain, of Breslau, to Mr Langley, of Cambridge, and to Dr Bradford, of University College, much still remains to be done. The physical and chemical process which fifteen years

ago seemed so beautifully clear,—the blood, the basement membrane, and the secreting protoplasm, regulated by its vaso-motor and secretory nerves,—though a true, is now found to be an inadequate physiological conception. The unexpected results of partial removal of the kidneys and of complete removal of the pancreas, in animals more closely allied to man than frogs or rabbits, offer new problems, we may almost say new paradoxes in physiology which seems as hard and, so to speak, illogical as any that we meet with in disease.

At present we are ignorant of the causes of either jaundice or cholæmia, as ignorant as of the true ætiology of uræmia.

This much, however, seems to be clear, that certainly most, and probably all, glands do much more work than secretion. When Bernard proved the glycogenic function of the liver, he showed that beside secreting bile it has a most important share in the metabolism of the body. Since then we have learned that the formation of urea has part of its process seated in the same great organ, so that it is probably as important in the transformation of proteids as of carbohydrates. And now two other functions, those of the accumulation of fats and of the formation of blood-corpuscles, are more than probably associated with the liver.

One used to explain these facts by the liver being in structure and vascular supply different from other secreting glands; but the effect of removal of the

pancreas in producing glycosuria, and the effect of removing the greater part of the kidney in producing polyuria with increased formation and excretion of urea, shows us that glands constructed apparently on a purely secreting type have other functions, metabolic, destructive, or inhibitory, the loss of which profoundly affects the vital processes of distant parts of the body.

But, in truth, this is only what we ought long ago to have learnt from the familiar case of the testis. Castration is the most ancient of physiological experiments performed in countless numbers, and even in this day of rebuke and blasphemy, carried out without license, certificate, or any legal restriction, and without anæsthetics, so long as the object is only the gratification of the palate or the convenience of life. Though forbidden for the trivial and unworthy objects of enlarging human knowledge and ultimately relieving human suffering, such experiments performed for other purposes are no less instructive. The production of fat in the ox and the capon, the altered shape of the gelding, and the modifications of the teeth and horns and antlers in boars and rams and harts—all of which changes depend on the activity or the abeyance of the genital glands—prove how much other work the testis does beside secretion of semen.

Lardaceous Disease

As to the causes of lardaceous degeneration of the liver, kidneys, and other organs, our experience in this country seems to prove that they are far fewer and more definite than is commonly believed by German pathologists. The lardaceous degeneration is certainly not produced by cachexia in general, nor by that of malaria or cancer in particular. Moreover, we may say, from its absence in the great majority of such common diseases as syphilis and phthisis, that it is not directly the result of the ill-health produced by either of these diseases. It belongs only to the later stages of syphilis, and to the most chronic forms of phthisis. It is only associated with disease of the bones or with cancer when they are attended with protracted suppuration, and it is found in cases of empyema and of accidental suppuration elsewhere, which have no so-called constitutional origin.

In my cases* syphilis, phthisis, and caries of bones were the three most frequent antecedents; but in all belonging to the two latter groups, and in nearly all of the first, long-continued suppuration was present, while there were many cases of suppu-

* See Appendix. The first table was drawn up by Mr. Lankester and published by Dr Fagge in his contribution to the debate on lardaceous disease which took place at the Pathological Society in the year 1876. The second table was compiled for me by Dr H. J. Campbell. The two tables together give the result of the post-mortem examinations held during a consecutive period of thirty years at Guy's Hospital.

ration without phthisis, syphilis, or caries. The conclusion seems inevitable that the cause of the disease is, as Dr Dickinson long ago maintained, continued loss of pus.

Nevertheless there are cases of general tuberculosis, and others of syphilis, in which we have no evidence of suppuration. There are also occasionally cases of prolonged suppuration in which no lardaceous disease results. As one, I may mention that of an elderly lady who had been for seventeen years the subject of an empyema with an external opening which daily discharged pus. In her last illness, when she died from bronchitis, at the age of sixty-six, I found the liver and spleen unenlarged, and the urine perfectly normal.

Unless we argue like one maintaining a thesis, we must not try to get rid of the troublesome exceptions to any rule that we may discover.

The case is, after all, what we are well accustomed to in pathology and in natural history in general. No symptoms are pathognomonic, no *presagia mortis* are infallible. The great saying of Bishop Butler that probability is the guide of life applies with striking truth to pathological and to therapeutical arguments. In medicine "always" and "never" must not be taken in their literal sense. If a statement is true of nine cases in ten, it is practically valuable and theoretically important. The remaining exceptions must not be denied nor explained away; but, on the other hand,

their presence must not lead us to neglect the remaining nine tenths.

Even in the comparatively simple and exact science of chemistry the most refined and laborious experiments, repeated again and again by eminent observers in France and Germany, in America and in England, fail to confirm the exact truth of the familiar formula H_2O; the results come so near the theoretical value that most men cannot help believing that the discrepancy is an accident. Nevertheless the third place of decimals remains intractable.

Diseases of the Kidney

The most important and interesting of renal affections is that which bears the name of our illustrious countryman Dr Bright; a disease, as I still venture to call it—not a group of diseases,—a disease presenting itself under many clinical aspects, and associated with divers and curiously interlaced anatomical changes; amid all which variety we can distinguish *two* main prominent and unquestionable types, around which numerous subordinate varieties group themselves with more or less individual distinction. In their ætiology, scarcely less than in their clinical aspects, their symptoms, their dangers, and their treatment, these two groups are at once distinct and yet closely related. The more acute and inflammatory form of Bright's disease with its characteristic anasarca

and condition of the urine, the form associated with the large smooth white kidney, has an equally characteristic ætiology. No one can doubt its frequent origin as a sequel of scarlet fever, and probably this connection has been rather under- than over-estimated. Whether it is ever the result of other specific fevers, of measles, smallpox, enterica, or diphtheria, is doubtful. I have myself only once seen the albuminuria of diphtheria pass into that of chronic Bright's disease. The rule is certainly that it entirely disappears, and we constantly see febrile albuminuria (in pneumonia, erysipelas, enteric fever) without any symptoms of renal disturbance at the time, or organic disease in the sequel.

When not a sequel of scarlet fever, acute Bright's disease may often be traced to a directly precedent exposure to cold, particularly to cold and wet. Moreover many cases in women undoubtedly originate in the prolonged renal congestion of pregnancy.

With respect to the most chronic form of Bright's disease, that associated with the small rough red kidney, many of its causes are known, although none are constantly and exclusively present. Thus it is often associated, as is generally admitted, with gout, with plumbism, and with spirit-drinking. But the last connection is certainly not so close as that between intemperance and cirrhosis of the liver. The rarity of the disease in early life, and its frequent association with degeneration of the arteries, lead one to regard it as in some sense

a senile malady; and there is certainly a similarity between the cachexia produced by drink and the symptoms of premature age. Yet we sometimes see typical cases of cirrhosis of the kidney with wasting and anæmia, œdematous conjunctivæ, hard pulse, and hypertrophied ventricle, in those who are comparatively young; and occasionally I have seen its presence in children not only probable by the symptoms, but proved by the autopsy. On the other hand, in examining the urine of healthy old people we often find it of good colour and specific gravity, without a trace of albumen; and the kidneys of those who have died above seventy years of age are often found perfectly unchanged. In this, as in other cases, it seems probable that the chronic inflammations, the degeneration, and the atrophy, which are apt to appear between sixty and seventy, bring about a fatal termination of any accidental malady; but if life is lengthened beyond seventy, without such indications of decay, the liability to them often ceases, and there remain only the non-inflammatory forms of atrophy, which mark extreme old age, but do not apparently hasten its end.

Another cause of cirrhosis of the kidney is mechanical interference with the excretion of urine, such as leads to hydronephrosis by the same hydraulic pressure as produces dilatation of the cavities of the heart, of the gall-bladder and biliary passages, of the stomach or the colon, on the proximal side of the mechanical obstruction. Hence this

form of renal cirrhosis was well called consecutive Bright's disease by Mr Marcus Beck. The conditions which give rise to this form of disease in men—stricture, enlarged prostate, and other causes of obstruction in the ureter—are well known. In women a more frequent cause seems to be prolapse of the uterus, and other local affections which drag upon the ureters and interfere with the passage of urine into the bladder.

Diseases of the Skin

We come now to the last group of diseases with the ætiology of which I propose to deal, those which affect the skin. All the ghosts of dead systems of medicine, all the idols of the forum and the cave, all the baseless hypotheses and unproved conjectures that have met us in other parts of our subject, are crowded together here. The subject was long the prey of specialists, and, far remote from the progress of physiology and pathology, was obscured by an antiquated and barbarous nomenclature, by pedantic distinctions between trivial varieties, and confusion of broadly differing pathological states ; while the treatment was theoretical, complicated, and ineffectual. Our eminent countryman Willan, however, was an accomplished physician, and viewed disorders of the skin from the point of view of the best general pathology and practice of his time. Accordingly his work, though scanty and imperfect, is, with few exceptions, accurate as far as it goes. The first

scientific treatise on the subject was constructed by Hebra on the principles of general pathology laid down by the pioneer in morbid anatomy, Rokitansky. Indeed, the chief fault of Hebra's great work is that he follows too closely, almost slavishly, the footsteps of Rokitansky. Since then the application of the microscope to morbid pathology, and the discovery of the true part played by parasitic fungi and invading bacteria have cleared up many obscure problems; and the observations recently made in this department, in this country and America, as well as on the Continent, are a worthy continuation of the scientific progress begun by Hebra. Still, however, we find traces of the old vices of method and of nomenclature lingering here and there. We still meet with such phrases in the ætiology of cutaneous diseases as "poverty of the blood," "acidity of the system," "a sluggish liver," "a dartrous diathesis," "nervo-sanguineous temperament," "scrofula" where there is no tubercle, "gouty tendency" where there is no gout, and "syphilitic taint" where there is no evidence of the presence of lues. Species and sub-species are still too many, and new diseases are too readily admitted; while even yet the principle which obtains in all other departments of medicine is not generally followed—that the name we give a disease should express its diagnosis and denote our best knowledge of its origin. To call a disease eczema without excluding scabies or fictitious dermatitis, to

apply the term erythema to every slight degree of traumatic dermatitis, or to diagnose psoriasis or lupus without excluding syphilis can but lead to inaccuracy of diagnosis and futility of treatment.

As in other departments of medicine, an ætiological arrangement of diseases of the skin is the best possible, and rightly supersedes all others, for it goes to the root of their pathology, and directs us to their prevention as well as their cure. Unfortunately, our knowledge of ætiology is here, as elsewhere, very imperfect, and even when a cause seems clearly ascertained, we find it is liable to such limitations from other causes, that it loses much of its importance.

Of these more or less fully ascertained causes, the most obvious is local, mechanical, thermal, or chemical irritation. The skin inflames when rubbed, when chafed by rough clothing, when exposed to a hot sun or a cold wind, or to salt water, to the presence of parasitic animals or plants, or to chemical irritants like cantharides. This reaction, which we may call common dermatitis, answers to the effects of cold air, dust, or other irritation on the conjunctiva, the fauces, or the bronchi, and to inflammation of the fauces by locally irritant poisons. The result scarcely differs, whatever the nature of the irritant: cantharides causes serous bullæ, and tartar emetic ointment a pustular eruption. But with these, and a few other exceptions, the difference in the result produced depends, first, on the

strength and duration of the irritant; and secondly, upon the vulnerability of the patient, a quality which varies widely with the individual, and is strikingly diminished by repetition of the irritation. This last quality the skin appears to possess in a far more marked degree than any other tissue, no doubt because it is so much more frequently exposed to irritants. The face and hands very soon become accustomed, by exposure, to heat and cold and wet, which at first excited violent reaction. The habitually covered parts of the skin are less readily hardened, and it is doubtful whether the fauces and the bronchial mucous membrane, different as is its susceptibility in different persons, can become at all inured to the irritation of cold dry air or of dust by frequency of exposure, while the internal organs seem to be almost destitute of this power of adaptation. Thus poisoning by lead does not bring tolerance, but gradual aggravation, and the joints never become accustomed to the presence of crystals of urate of soda. Nor does it seem that the liver becomes accustomed to the presence of alcohol in the portal blood, but rather inflames in exact response to the frequency and strength of the irritation. There are, however, exceptions which show that even the internal organs, and particularly the nervous system, may under some conditions, as a result of habit, cease to react to stimulants, not less remarkably than the skin itself. The amount of alcohol which would cause vomiting or coma in

a youth will scarcely affect the seasoned toper. The schoolboy's experience of the effects of smoking tobacco is seldom repeated after it has become a habit, although occasionally, under special circumstances, it may reassert its power, showing that its physiological effects are masked or inhibited rather than abolished. A like tolerance, as we call it, is to be observed as the result of the frequent exhibition of various drugs, of which opium is the most striking example. It is, however, observable that these cases of "hardening" of the internal organs relate for the most part to agents acting after absorption upon the nervous system, and not to direct irritation.

It was one of Hebra's merits that he showed how an exuding dermatitis indistinguishable from eczema may be produced at will by applying irritants to the healthy skin; but I venture to think that an equally important fact was overlooked by him when he called this "artificial eczema," and included scabies as a kind of eczema; namely, that in the great majority of cases of eczema the skin reacts *as if* it had been irritated, but no evidence of an irritant can be found. It is these cases of idiopathic eczema which surely must be taken as the type of the disease, and we gain little light as to their ætiology or treatment from observation of traumatic cases. The best way to put the facts seems to be to describe the results of direct irritation as "common superficial dermatitis of traumatic origin," and to define

eczema as an "idiopathic common superficial dermatitis," with a special local distribution, course, and natural history which distinguish it as a separate disease. Nevertheless it is important to bear in mind the anatomical resemblance between the purely secondary and the purely idiopathic cases, to recognise that the latter have not unfrequently their origin in the former, and that there are innumerable gradations of responsibility to be adjusted between the *irritabile* and the *irritans*.

If we pass from eczema to its nearest allies, we find that impetigo of the scalp in children is often the immediate result of the irritation of pediculi, and of the scratching to which their presence gives rise; that prurigo senilis is no less the result of the itching and scratching caused by *pediculi vestimentorum;* and that scabies is nothing but common superficial dermatitis, papular, vesicular, or pustular in degree, produced again partly by the presence of the parasite, and partly by the efforts of the patient to relieve the pruritus thus caused. In the case of pustular scabies and impetigo of the scalp we have now a second ætiological factor to consider, namely, the contagiousness of the pus secreted, dependent, without doubt, upon the presence of more than one variety of microphyte. This explains the spread of the disease, as pus is conveyed to distant parts by the patient's fingers, and many cases of impetigo and so-called eczema of the trunk are thus readily explained.

Another good example of the contagion of pus is afforded by the multiplication of furunculi. There is little or no ground for the opinion that they depend upon "poverty of the blood" or any other "constitutional" cause. The accidental inflammation of a deep hair-sac leads to a minute spot of gangrene and acute surrounding suppuration, and from this adjacent hair-sacs are infected, particularly when the surrounding skin is softened and a transfer of pus rendered easy by the use of warm and moist poultices. The efficacy of a disinfectant and astringent treatment confirms the same view.

These, then, appear to be the chief external demonstrable causes of dermatitis:—local friction, heat and cold, wet and dryness, the irritation of scratching, the irritation of animal parasites, and the contagion of pus. To these we must add the presence of microscopic fungi invading the epithelium, the hairs, and the hair-sacs, and producing ringworm, favus, and the other varieties of tinea.

We now come to a second cause of dermatitis, namely, poisons not external, but conveyed by the blood, and acting chemically, not mechanically. These may, I think, be combined in a natural group, that is to say, one in which a common ætiology is associated with common anatomical and clinical characters, distribution, course, and treatment. Just as traumatic dermatitis is the type of the group to which eczema, impetigo, and scabies belong, so the type of erythematous dermatitis is

the eruption called forth by copaiba, belladonna, and some other drugs. Under this group we may include the various forms of erythema combined under the name multiforme by Hebra, the roseola of authors, urticaria, and some of the cases which have been described as herpes, hydroa, acute pemphigus, and dermatitis herpetiformis. Their anatomical character of superficial dermatitis, not leaving scars, is that of eczema and its allies, but they differ therefrom in several particulars. First in anatomy: the inflammation never forms permanent papules, nor small vesicles, nor pustules and crusts, nor weeping surfaces; while, on the contrary, it is almost always accompanied by subcutaneous œdema, frequently by wheals, and sometimes by hæmorrhage. Moreover its papules are transitory, and its vesicles large or bullous. Secondly, it is most certainly produced not by mechanical or thermal, but by chemical irritation, by the poison of a bee's sting or a gnat's bite, or the secretion of a nettle. Again, it is never chronic in course, but acute or subacute, fugacious and recurrent. It produces smarting rather than itching. It is not nearly so symmetrical as eczema or psoriasis, and its favourite localities are different from those of any other kind of dermatitis—the forearm and back of the hand, the shin and instep, while it may spread diffusely over the trunk and face as an acute exanthem.

These erythematous rashes may often be traced to intoxication with some poison from within. Thus

we may compare the erythema or urticaria produced by copaiba with that which follows the ingestion of mussels, crabs, and certain other articles of food. Still more frequently the eruption follows gastric irritation from more ordinary causes, in which what is as a rule wholesome food becomes to the individual or on a particular occasion poisonous in its effects.

The view now advocated has been lately supported by the erythematous rash which sometimes follows injection of the antitoxin of diphtheria, for the eruption is not confined to the seat of injection, and resembles idiopathic erythema in distribution.

Another ætiological factor in the production of erythema is rheumatism. This is proved by the not infrequent appearance of erythema while the joints are still inflamed and the fever is high; secondly, by *erythema nodosum* so frequently recurring in those who have suffered once or more from rheumatism; and thirdly, by the hæmorrhagic eruption called *peliosis rheumatica*, bearing all the characters of a true erythema.

The last ætiological point in relation to this erythematous group is its more frequent occurrence in children and young adults than in elderly persons; and, among adults, in women more than in men.

There is a well-known affection of the skin, formerly called *acne rosacea*, which is totally distinct in anatomy, distribution, and pathology from true acne.

The latter consists in obstruction, retention, and inflammation of the sebaceous glands. Its locality is the face, shoulders, and upper part of the chest, and it always begins about puberty. It is no doubt closely related in its ætiology to the development of the hair-sacs and sebaceous glands which attend the appearance of the beard; and we must, I suppose, explain its being as common among maids as among youths by the fact that, although the former have no beards, they are descended from fathers who had them.

Gutta rosea, as the other affection is now generally called, only accidentally and occasionally affects the sebaceous glands in common with the other structures of the affected area; it occurs much later than the time of puberty; it is confined to the nose, the chin, the cheeks, and the ears; and its ætiology is related to reflex disturbance of the vaso-motor nerves. Some persons, after taking food, suffer from dilatation of the blood-vessels of the face, producing a sensible redness and heat which they recognise as flushing. If frequently repeated, this slight erythematous blush is followed by dilatation of the small veins, by œdema, and at last by hypertrophy; the latter process being comparable to the clubbing of the fingers and toes which results from the chronic venous congestion of cardiac disease. The fiery red and swollen nose, with its "bubuckles and welks and knobs and flames of fire," has been held, since Bardolph's time, to be the consequence

of drink, and no doubt it often is; but many drunkards carry no such "lantern in the poop;" and it would be most unjust to assume that every patient is intemperate who is afflicted with this unsightly complaint. It is commonly produced by gastric irritation, and the most frequent cause of this is alcohol: but anything which leads to flushing of the face will produce the same result; and in some persons, particularly women at the climacteric period, any meal, however plain and scanty, is apt to be followed by the flushes referred to. Where there is no dyspepsia to account for this result in women, we can almost always trace the cause of gutta rosea to ovarian irritation, proved by its aggravation at the menstrual periods and subsidence in the interval. Indeed, the two sources of vaso-motor disturbance not infrequently coincide in the case of women between forty and fifty.

Another cause of dermatitis is undoubtedly to be found in nervous conditions. We have clear evidence of the effect of lesions of the trophic nerves upon the skin in the frequently observed cases of bullæ and sloughing ulcers which follow injuries to one of the nerves supplying the upper or lower extremity. A large number are recorded in Dr. Weir Mitchell's well-known work, and many additional ones in Mr. Bowlby's Astley Cooper Essay.*

There is now little if any doubt that the slough-

* Afterwards published with important additions under the title 'Injuries and Diseases of Nerves' (Churchill, 1889).

ing of the cornea which follows injury to the fifth nerve, and the acute sloughing bedsore which so speedily follows certain cases of severe myelitis, are also trophic lesions.

One form of acute dermatitis certainly belongs to this ætiological group,—that, namely, which used to be called herpes zoster, and is or ought to be called zona. But we have also the condition described by Sir James Paget as "glossy fingers," and the bullous and congestive dermatitis which is occasionally seen as the result of peripheral neuritis or of pachymeningitis cervicalis.

I have attempted, Sir, so far as the limits of my time would allow, to direct attention to the known and immediate causes of disease, and to the less direct predisposing or determining causes, such as climate and race, occupation, sex, and age, which modify the effect of external irritants and even of contagion. Much of what I have brought before you has been criticism—some of it, I hope, destructive criticism—of current ætiological explanations, but this seemed to be necessary, for it is only when the ground has been cleared that we can hope to build.

THE

HARVEIAN ORATION

DELIVERED AT THE

ROYAL COLLEGE OF PHYSICIANS ON ST. LUKE'S DAY

1893

THE HARVEIAN ORATION, 1893

It is now 237 years since the illustrious Fellow of this College whose name we are met to commemorate, provided, when two years before his death he conveyed his estate at Burmarsh to the College, that—

"There shall be once every year a general feast for all the Fellows; and on the day when such feast shall be kept, some one person of the said College shall be from time to time appointed by the President and two Eldest Censors and two Eldest Elects for the time being (so that the person so to be appointed be not in that behalf appointed two years together), who shall make an Oration publicly in the said College, wherein shall be a commemoration of all the Benefactors of the said College; with an exhortation to the Fellows and Members of the said College to search and study out the Secrets of Nature by way of Experiment; and also for the honour of the profession to continue in mutual love and affection among themselves, without which neither the dignity of the College can be preserved, nor yet particular men receive that benefit by their admission into the College which they might expect; ever remembering that '*concordiâ res parvæ crescunt, discordiâ magnæ dilabuntur.*'"

It will be seen from this quotation that there is no obligation on the orator to commemorate Harvey alone, or at all, except as one of the many benefactors of the College; and inasmuch as the material benefits of the gifts conferred by Linacre and

Caius, by Harvey and Hamey, and by the founders of our College lectures, are less valuable than the intellectual gifts which have led Fellows of the College since Harvey's time to search out "the secrets of nature by way of experiment," and still less valuable than the mutual respect and affection among ourselves, by which the honour of the profession has been advanced and the dignity of the College preserved, it would be in accordance with my duty to-day to recall to your memory the scientific achievements of Gilbert, of Glisson, or of Willis, of Jurin, of Thomas Young, of Wells, or of Prout, or the more strictly medical labours of Sydenham, or Heberden, or Bright. Nor less worthy of commemoration would be those Fellows who have dignified our community by their literary genius, as Arbuthnot; or by their taste and munificence, as Mead; or by the humanity and simplicity of their character, as Babington, and Watson, and Parkes, and Wilson Fox.

The tradition, however, handed down for so many years seemed too strong to be broken, and I therefore invite you once more at this Harveian Festival to consider some aspects of the work of Harvey.

I. Concerning that immortal discovery which places him in the limited class represented by Aristotle and Archimedes, Copernicus, Newton, and Darwin, it is difficult to say anything that

has not been better said already; for again and again its originality and importance, the methods by which it was attained, the steps made by others which led up to it, and the effects which followed it, have been learnedly and eloquently expounded by my distinguished predecessors.

To refute, however, all cavils against the priority of Harvey, and all attempts to transfer his laurels to the head of another, it is sufficient to bear in mind the following considerations:

1st. If Harvey's doctrine of the circulation was not new, why was it opposed by men in the position of Riolanus and Hoffmann, and welcomed as a discovery by Bartolinus and Schlegel and Descartes? Surely his contemporaries were better judges of the novelty of his views than we are!

2nd. Admitting that Servetus and Columbus taught the doctrine of the lesser circulation, we need but a moment's thought to convince us that no complete knowledge of this part of the subject was possible until the existence of a systemic circulation was established; for the one is physically impossible without the other.

3rd. The title of Harvey's great work is not, as it is sometimes quoted, 'The Circulation of the Blood,' but '*De Motu Cordis et Sanguinis.*' He first showed that the flesh, or parenchyma, of the heart is true muscle; that the heart is not a passive chamber receiving the blood, but a contractile organ expelling it. Until the motive power

of the heart was understood there *could* be no true theory of the circulation.

The fact is, that when we know the true solution of a problem, it is easy to see or think we see it in any discussion which preceded the discovery; for there is only a limited number of answers to most questions, and therefore true as well as false solutions are almost sure to have been proposed.

In the writings of Columbus, Servetus, and Cæsalpinus, phrases occur which sometimes seem as if the writers were going to state the truth that Harvey first asserted.

But it would be as reasonable to infer, from such passages, that the circulation of the blood was then known, as from the lines that Shakespeare puts into the mouth of Brutus:

> "As dear to me as are the ruddy drops
> That visit my sad heart."

Many such passages occur in writers of the sixteenth century. But I venture to say that no passage like the following one, taken from Dryden's play of 'Don Sebastian,' will be found in a work written before the publication of Harvey's discovery:

> "Now, Heart,
> Set ope thy sluices! send the vigorous blood
> Through every active limb for my relief;
> Then take thy rest within thy quiet cell,
> For thou shalt drum no more."

As Paley well said, "he only discovers who proves." To hit upon a true conjecture here and

there amid a crowd of untrue, and leave it again without appreciation of its importance, is the sign, not of intelligence, but of frivolity. We are told that of the Seven Wise Men of Greece, one (I believe it was Thales) taught that the sun did not go round the earth, but the earth round the sun; and hence it has been said that Thales anticipated Copernicus—a flagrant example of the fallacy in question. A crowd of idle philosophers arguing through the long summer days and balmy nights of Attica about all things in heaven and earth must sometimes have hit on a true opinion, if only by accident; but Thales, or whoever broached the heliocentric dogma, had no reason for his belief, and showed himself not more, but less reasonable than his companions. The crude theories and gross absurdities of phrenology are not in the least justified, or even excused, by our present knowledge of cerebral localisation; nor do the baseless speculations of Erasmus Darwin and Lamarck entitle them to be regarded as the forerunners of Erasmus Darwin's illustrious grandson. Cuvier was perfectly right in his controversy with Geoffroy St. Hilaire, for the weight of evidence was on his side. Up to 1859 impartial and competent men were bound to disbelieve in evolution; after that date, or at least so soon as the facts and arguments of Darwin* and Wallace had been published, they

* 'The Variation of Animals and Plants under Domestication' was published in 1868.

were equally bound to believe in it. He discovers who proves; and by this test Harvey is the sole and absolute discoverer of the movements of the heart and of the blood.

Concerning the *methods* used by Harvey, they were various; and his discovery, like most great advances in knowledge, was not achieved by one of the happy accidents which figure in story-books, or by the single crucial experiment, never to be repeated except under licence and special certificate, which some members of a certain Royal Commission supposed to be the only kind of experiment needed in scientific inquiries.

A perusal of Harvey's own statements makes it plain, it seems to me, that having gained his knowledge of the anatomy of the heart and of the current hypotheses of its function from his Italian masters, he reasoned thus: that the cardiac valves must be intended for such physiological service as their construction indicates. He believed that every part of this human microcosm has a meaning—that it is by no chance result of blind forces that an organ is adapted to its end. This great postulate is necessary for scientific progress. If the difficulties of physiology, whether normal or morbid, seem so intricate and insuperable that we are tempted to doubt whether the riddle after all has an answer, we must again and again fall back on the faith of Harvey and of Newton, of Boyle and of Linnæus. The great doctrine of natural selection has

thrown wonderful light upon the methods by which the results that we see have been reached, but has not impaired the excellence of those results nor their evidence of beneficent design. The application of scientific methods to the study, not only of man as an individual, but to the human race in its social aspects—the science of civilisation in its ethical and political development—that *nova scientia* which was foreseen by Harvey's contemporary, Vico—has so enlarged our conceptions that we may invert the argument of the Roman orator when he inferred Providence in human affairs from design in human structures:

"Est, est profecto illa Vis, neque in his corporibus atque in hac imbecillitate nostrâ inest quiddam quod vigeat et senteat, et non inest in hoc tanto naturæ tam præclaro motu."

Belief, then, that the body and all its parts is a machine constructed for certain uses, that everything in nature has a reason and an end—this was Harvey's postulate when he argued out the functions of the heart and vessels from their anatomical construction.

His second method was that of actual experiment. On this point he leaves us in no doubt. His second chapter is headed "*Ex vivorum dissectione qualis sit cordis motus*," and in the introductory chapter which precedes this he says:

"Tandem majori indies et disquisitione et diligentia usus, multa frequenter et varia animalia viva introspiciendo, multis observationibus collatis, et rem attigisse, et ex hoc labyrintho me extricatum evasisse, simulque motum et usum cordis et arteriarum quæ desiderabam comperta habuere me existimabam."

Many of his vivisections were not strictly speaking experiments, but observations—inspection of the living heart and arteries; others were experiments in the modern and restricted use of the word. These were Harvey's methods, as they must be the methods of all natural science: first, observation; next, reflection; then experiment. "Don't think; try," was Hunter's advice to Jenner,—an advice that is often needed by an acute inquiring genius like his; still more often by sheer idleness, that will never bring its fancies to the test of fact.* Cowley puts the same precept thus:

> "Thus Harvey sought for truth in Truth's own Book,
> The creatures, which by God Himself was writ,
> And wisely thought 'twas fit
> Not to read comments only upon it,
> But on the original itself to look."

Experiments without hypotheses are often fruitless, but hypotheses which are never brought to the test of experiment are positively mischievous.

How far have the Fellows of this College obeyed Harvey's precept and followed his example in "searching out the secrets of nature by way of experiment"? We must, I fear, confess that after the brilliant period of the seventeenth century (in some respects the greatest of our history, and certainly the most fruitful in great men) experimental science made slow and uncertain progress,

* Ea autem vera esse vel falsa, Sensus nos facere debet certiores, non Ratio; *αὐτοψία*, non mentis agitatio. (Second Epistle to Riolanus, p. 133, College Edition.)

so that between Harvey and Newton, Hooke and Grew, Mayow and Boyle on the other hand, and Cavendish, Black and Priestley, Hunter and Hewson on the other, there was a long period of stagnation or even retrogression. Hypotheses and dogmas, misapplied mathematics, imperfect chemistry, and an affected literary style (made more conventional by the practice of writing in a foreign language better fitted for rhetoric than science) contributed to make the eighteenth century comparatively barren in so far as science generally, and physiology and medicine in particular, are concerned.

It is a remarkable fact that Dr. Gregory, of Edinburgh (1753—1821), in his once highly valued 'Conspectus Medicinæ,' writes thus uncertainly and on hearsay of the ordinary facts of hemiplegia, which had been well known to Aretæus:

"Fertur, et sane plurimorum jam medicorum observationibus confirmatur, latus adversum ab eo in quo cerebri vitium est sic resolvi." (Chapter xii, section 382.)

Heberden, it is true, contributed observations which were not unworthy of Sydenham or Hippocrates, but his work, like theirs, was purely clinical. It was not until the close of the Great War that scientific medicine made a fresh start. Its progress has since mainly depended upon the application of new methods of observation by the stethoscope, the test-tube, the microscope, the clinical thermometer, and the ophthalmoscope.

The "way of experiment," in the strict sense of the word, has been hitherto most successfully applied to normal physiology. The successors of Harvey were not Sydenham, Radcliffe, Arbuthnot, Garth, Meade, Freind, and Heberden, but Lower, Mayow, Hales, Vierordt, Ludwig, and Chauveau. Pathology as an experimental science is still in its infancy, but the infancy is that of Hercules, and bids fair to strangle such dire pests as anthrax, cholera, tetanus, and hydrophobia.

Before quitting this part of my subject I would fain correct a popular misconception that Harvey was a neglected genius—that his contemporaries, his professional brethren, and in particular this ancient College, refused to listen to his new notions, ridiculed his discoveries, and spoiled his practice. Whether as his fame grew his practice diminished we cannot tell. If so, his patients were the losers. What Harvey and every honest man cares for is not popular applause, but the confidence and esteem of his comrades, and this he deserved and received. It was as lecturer at this College that he propounded his discoveries; it was here that he found his disciples and his friends. Here he was urged to take the Presidential chair; and here his statue was erected, five years before his death, with the inscription "*Viro monumentis suis immortali.*" It would have been a poor compliment to his elaborate demonstrations, and unworthy of a liberal profession, if so

startling a revolution as Harvey proposed had been accepted without inquiry. It was considered, it was discussed, and without haste, but without timidity, it was at last accepted—the very way in which Darwin's theory was received and criticised —and finally adopted by Lyell and by Hooker. Let, then, no scientific impostor or medical charlatan quote Harvey to console him under merited censure.

II. Of Harvey's writings the second, and by far the longer treatise is that upon 'Generation.' This formed the subject of a valuable criticism in the Harveian Lecture by the late Dr. Arthur Farre. It is full of interest, and contains many observations that remain true for all time, many acute criticisms, and a few broad and true generalisations, such as the famous dictum, "*Omnia animalia ex ovo progigni.*"

Some passages show that Harvey was not without the faculty of humour, which, as Dr. Arnold remarked, few great men have lacked. Such is the account of the accomplished parrot who was Mrs. Harvey's pet, and through a long life maintained the masculine character, until in one unguarded moment she lost it and her life together.

Perhaps, however, what most strikes the reader of this treatise is the learning of the writer. He is familiar with his Aristotle, and quotes from Fabricius and other writers with much greater freedom than in the succinct and almost sententious treatise, 'De

Motu Cordis et Sanguinis.' Some would have us believe that here, as in other cases, erudition was a clog upon genius. This question has been often discussed, and it has even been maintained that he is most likely to search out "the secrets of nature by way of experiment" who comes fresh to the task with his faculties unexhausted by prolonged reading, and his judgment uninfluenced by the discoveries of others. This, however, is surely a delusion. Harvey could not have discovered the circulation of the blood had he not been taught all that was previously known of anatomy. True, no progress can be made by mere assimilation of previous knowledge. There must be intelligent curiosity, an observant eye, and intellectual insight.

Doctrina sed vim promovet insitam;

and few things are more deplorable than to see talent and industry occupied in fruitless researches, partially re-discovering what is already fully known, or stubbornly toiling along a road which has long ago been found to lead no whither.

We must, then, instruct our students to the utmost of our power. Whether they will add to knowledge we cannot tell, but at least they shall not hinder its growth by their ignorance. The strong intellect will absorb and digest all that we put before it, and will be all the better fitted for independent research. The less powerful will at least be kept from false discoveries, and will form

(what genius itself requires) a competent and appreciative audience. Even the dullest scholars will be respectable for their learning, and if they cannot make discoveries themselves, can at least enjoy the delight of intelligently admiring the discoveries of others.

III. There is, however, a third phase of Harvey's intellectual work, of which, unfortunately, the records have perished, and which has not, perhaps, been duly appreciated. I do not speak of his practice. A file of his "bills," however, interesting to the antiquary, would probably be of as little therapeutical value as those of his contemporaries. What I believe Harvey contributed, or would, but for adverse fate, have contributed to medicine as distinct from physiology, was a systematic study of Morbid Anatomy.

In the following passage he speaks of the great benefit that would ensue from the regular observation of the structural changes produced by disease:

Sicut enim sanorum et boni habitûs corporum dissectio plurimum ad philosophiam et rectam physiologiam facit, ita corporum morbosorum et cachecticorum inspectio potissimum ad pathologiam philosophicam. (P. 92 of 4to College Edition.)*

* "In the same way that the dissection of healthy bodies is of the greatest importance for true scientific physiology, so the inspection of diseased and infirm bodies after death is no less necessary for scientific pathology.

"For physiology is the study of natural conditions, and must first be studied by physicians, inasmuch as what is normal is healthy, and the rule for its own rightness, as well as for every abnormal

Now this was a new notion. It was not uncommon for the body to be opened after death, especially in the case of great personages, either for the purpose of embalming or for discovering (as it was supposed) the fact of poison or other foul play; and occasionally a physician would obtain permission for a like inspection when something unusual in the symptoms had excited a laudable curiosity to ascertain their cause. But the records of such inspections in the seventeenth century by Bartolinus, or Tulpius, or Bonetus, or, in our own country, by Mayerne, or Bate, or Morton, are fragmentary, their object being limited to the individual case. There was no attempt to search out the secrets of nature in disease by a systematic observation of the state of the organs after death, nor was there for more than a century after Harvey's death. Morgani* in Italy; the French anatomists of the early part of this century, Corvisart and Laennec, Broussais and Cruveilhier; in Germany Meckel and Rokitansky; and in England Baillie, Abercrombie, Carswell, and Bright, —these were the founders of scientific pathology

deviation therefrom. But when deviations from health or any abnormal conditions are defined by the light of healthy structures, then pathology, the science of disease, becomes intelligible. Then also from a knowledge of pathology the practice and art of healing, and numberless new methods of treatment, will naturally spring." For records of autopsies, see Exerc. ii ad Riolan., pp. 113, 127.

* John Baptist Morgani died at Bologna in 1771; his great work, '*De sedibus et causis morborum per anatomen indagatis*,' was published in 1761.

on a sure anatomical basis almost within living memory.

Not only had Harvey the prescience to recommend the study of Morbid Anatomy for itself, but he had himself carried it out by recording dissections, or, as we should now call them, inspections, of diseased bodies. Most of these post-mortem reports, with his observations on the generation of insects, and other manuscripts were destroyed, or irrevocably dispersed, when his house in London was searched while he was with the King at Oxford, and others have only lately been brought to light by the publication of his manuscript lectures. If the records of these inspections had been published, may we not assume that Harvey's great authority would have set the fashion, and that the systematic study of Morbid Anatomy would have begun a century and a half earlier than it did? And think what this would have meant. With the exception of a few shrewd observations, a few admirable descriptions, and here and there a brilliant discovery, such as the origin and prevention of lead colic and of scurvy and the introduction of vaccination, it may be said that medicine made no important progress between the time of Harvey and that of Laennec. The very notion of diagnosis in our modern sense of the word depends upon Morbid Anatomy. The older physicians seldom attempted to determine the seat of an ailment.*

* I was once told by a contemporary of an eminent physician in

Disease was looked upon not as a condition depending upon disordered physiological functions, but as something external, attacking a previously healthy person, disturbing, and, if not expelled by art, finally destroying him; while any structural changes which were found after death were regarded rather as the effects than the causes of the symptoms during life.

Now, the ambition of every intelligent student—and in Medicine we are life-long students—is to fix upon the most objective, certain, and important of the symptoms of a patient, to follow out this clue, to determine the organ affected and the nature of the affection, so that in his mind's eye the tissues become transparent, and he sees the narrow orifice for the blood-stream and the labouring muscle behind it; or the spinal cord with grey induration of a definite region, and the motor, sensory, and trophic changes which physiologically ensue; or the constricted loop of intestine with violent peristalsis above and paralysis below, the blood-current stopped, and congestion passing hour by hour into gangrene.

Sometimes this minute search to fix upon the locality and exact nature of a lesion has been ridiculed; and we are asked what benefit to the patient such knowledge when attained can bring.

this city that the accuracy of his diagnosis was considered remarkable (it was before the introduction of the stethoscope), since he rarely failed to fix the seat of a disease in that one of the three great cavities of the body in which it was found after death.

We answer, that in Medicine, as in every other practical art, progress depends upon knowledge, and knowledge must be pursued for its own sake, without continually looking about for its practical application.

Harvey's great discovery (which we physicians rightly celebrate this day) was a strictly physiological discovery, and had little influence upon the healing art until the invention of auscultation. So also Du Bois Raymond's investigation of the electrical properties of muscle and nerve was purely scientific; but we use the results thus obtained every day in the diagnosis of disease, in its successful treatment, and in the scarcely less important demonstration of the falsehoods by which the name of electricity is misused for purposes of gain.

It is true that Bernard's discoveries of the diabetic puncture and of the digestive function of the pancreas have not yet received their practical application. He was right when he said, "*Nous venons les mains vides, mais la bouche pleine d'espérances légitimes*"—but he should have spoken of himself alone.

For the experiments on blood-pressure begun by Hales, and carried to a successful issue in our own time by Ludwig, have already led to knowledge which we use every day by the bed-side, and which only needs the discovery of a better method of measuring blood-pressure during life to become

one of our foremost and most practical aids in treatment.

Again, we can most of us remember when the attempt to fix the locality of an organic lesion in the brain was a mere scientific game, which could only be won after the player was beaten; for when the accuracy of diagnosis was established its object was already lost. But who would say this now, when purely physiological research and purely diagnostic success have led to one of the most brilliant achievements of practical Medicine—the operative treatment of organic diseases of the brain.

Morbid Anatomy not only teaches us what lesions we have to discover, but also their true pathological relation. We learn that every disease does not affect every part of the body. If we make a horizontal list of all the organs and tissues, and a vertical one of all the known morbid changes, according to Rokitansky's laborious and thoroughgoing method, we find the table almost useless, since the blanks are more numerous than the entries. If the liver, for example, were the possible seat of every tumour described in our College nomenclature of disease it would be hopeless to attempt the diagnosis of an hepatic growth; if the small intestine were subject to every morbid change which can affect a mucous membrane we should be lost amid such a crowd of possibilities. But Morbid Anatomy teaches us that each organ has its peculiar liabilities to disease, so that when we have determined the *seat* of a

malady we have gone far towards establishing its *nature*. Moreover, such concomitant variations as hypertrophied left ventricle and granular kidneys, basal meningitis and caseous lymph-glands, visceral abscess and ulceration of the cardiac valves, teach us much more than anatomy or diagnosis. They throw a clear light upon the nature and the origin of diseases, and hence upon their prevention and cure.*

True, there are functional disorders, and others which are toxic or parasitic; but it is the object of the study of Morbid Anatomy to limit and, as nearly as may be, to abolish the former group by tracing disordered functions to structural changes or to poisonous or parasitic influences. We may measure the progress of medical science during the present century by the fact that Fever, Dropsy, Paralysis, and Apoplexy are for us no longer diseases, but only symptoms which leave us unsatisfied till we have traced them to their origin.

It has often been questioned whether the study of Morbid Anatomy has not withdrawn attention from Morbid Physiology; and, again, whether the time employed upon pathological researches would not have been better spent in directly therapeutical inquiry. To both these questions I take leave to answer, No. Anatomy must precede Physiology, whether in the normal or the diseased state. The humoral physiology of the ancients did infinite

* See Dr Wilks's article in the 'Guy's Hospital Reports' for 1865 (3rd series, vol. xi, p. 1).

mischief (mischief not yet exhausted), because it lacked the sound basis of Anatomy; and Experimental Pathology, necessary and important as it is, and valuable as even its first endeavours have proved, was impossible without previous knowledge of the anatomy and histology of disease. As to Therapeutics, while professing full faith in the ultimate value of such laborious chemical and physiological researches of the laboratory as those which were the subject of the recent Croonian Lectures by Dr Leech, I hold that for the successful cure of a patient it is far better that his physician should have a thorough and extensive knowledge of Morbid Anatomy than that he should be acquainted with all the baths and waters, the hotels and lodging-houses throughout the world, or familiar with the barbarous names and pretended virtues of all the advertised nostrums that deface the fair English fields from London to Oxford. The public suppose that it is *their* business to know what is the matter, and the doctor's to find the remedy; if so, our art would be confined to learning the name of the patient's disorder by letter, post-card, or telegram, and looking up in an index of remedies the twenty or thirty drugs which are "good" for that particular complaint. We know that the real difficulty is to ascertain the nature and origin of our patient's disorder; when that is done the treatment in most cases is obvious, and in many effectual; when it is not done our treatment is vacillating, and either futile or mischievous.

We have already ample means at our disposal for influencing almost every organ of the body. A new tool is occasionally offered us which deserves proving, but what we want far more is knowledge how to use the tools that we have. Treatment without diagnosis, besides its inefficiency, brings us for the time unpleasantly near to the charlatan, who, whatever title he may assume, is always therapeutical, and never pathological. Rational, bold, and effectual treatment, whether preventive or curative, must always depend upon accurate diagnosis and sound pathology; and the power of diagnosis depends upon that systematic inspection of the bodies of diseased persons which was recommended and practised by Harvey.

"Ad" [hanc] "inspectionen, cum Heraclito apud Aristotelem,* in casam furnariam (sic dicam) introire si vultis, accedite: nam neque hic Dii desunt immortales." ('Excerc. ij ad Riolan,' p. 110.)

Suffer me, then, Mr. President and Fellows of this College, to obey the instructions of the founder of this Lecture, by exhorting my hearers, and especially those Fellows who are junior to myself, to emulate, according to the varied talents entrusted to each, the example of Harvey in these three particulars:

(i) In investigation by experiment, whether in pathology or physiology.

We have now difficulties unknown to Harvey in carrying out this duty, for duty it certainly

* Arist. de part. animalium, i, 5.

is, incumbent upon all who have the opportunity and the necessary training. The countless experiments on living animals which were carried out during the seventeenth century in all civilised countries—in Italy, Holland, Denmark, France, Germany, and England—bore a rich fruit of physiological knowledge. If the anatomy of the human body was thoroughly ascertained by the great men of the sixteenth century, by Vesalius, Sylvius, and their successors, it is no less true that to the seventeenth century is due the discovery of the elements of physiology. The action of the heart and the circulation of the blood, the absorption of chyle by the lacteals and thoracic duct, the mechanism of respiration, and some knowledge of its chemical effects, the function of secretion by glands, the minute structure of the eye and ear, and of the reproductive apparatus, and a knowledge—imperfect, but true as far as it went—of the functions of the brain and nerves—these were the achievements of the seventeenth century due to Harvey, Glisson, Willis, and Mayow, among our own countrymen, and to Pecquet, Malpighi, Leuwenhoeck, de Graaf, Swammerdam, Aselli, Redi, and Bartolinus. In all this brilliant advance of knowledge experiment upon the lower animals was the method used, and the method is as indispensable now.

Anyone conversant with a single branch of Natural Science is aware that experiment, as well as observation, is necessary. Who would expect

discoveries in physics or in chemistry without laboratories and experiments? Do not botanists investigate the functions of plants by dissection, by microscopic and chemical investigation, and by *experiment?* Have we not this very year celebrated the important results of fifty years' *experimental* researches into the life and growth of plants by Lawes and Gilbert? And is it not obvious that the same necessary well-tried and indispensable method of inquiry must be continued in the case of animals? Happily the same experimental science has discovered the means of abolishing the tribute of suffering which the brute creation paid in the hands of Harvey and Hales, of Haller, Magendie, and Sir Charles Bell. By means of chloroform and other anæsthetics, and by means of the antiseptic methods which we owe to Sir Joseph Lister, the subjects of experiment are spared the pain and shock of an operation, and the pain which used to follow an operation. In fact, almost the only experiments upon the lower animals which involve distress are those which are most immediately and directly useful to ourselves and to them; inoculations, namely, with a view to reproduce diseases and the direct therapeutical testing of drugs. Cruelty is utterly repugnant to our calling; and it seems absurd that men, who will with just confidence entrust themselves and the lives of those nearest to them to our protection and care, should yet so far distrust us as to shackle attempts to improve our

knowledge and our power by cumbersome and ridiculous restrictions. Let us hope that, on the one hand, increasing humanity and gentler manners will extend compassion for the lowest of God's creatures from the educated classes of England and America until it permeates all ranks and all nations; and that, on the other, full liberty will be given to the prosecution of researches, laborious and thankless in themselves, but of the utmost value for the relief and prevention of disease in man and brute alike. May I also express a hope that those who administer our laws will take heart of grace, and in this, as in other matters, try whether Englishmen do not prefer the conscientious maintenance of a Statesman's own judgment before a time-serving subservience to ignorant clamour.

(ii) In the second place I would exhort my brethren, and especially the Members of this College, to cultivate Learning. Harvey went to study in Italy, then the nursery of science as well as of art, and he was familiar with the writings of Plato and Aristotle and Virgil, as well as with those of his immediate predecessors, Fabricius and Columbus. So in that golden time which comes to most of us between taking the Academical Degree and becoming immersed in the daily duties of hospital life, I strongly advise a visit to one of the German universities, or to Paris, to acquire the key to the two languages in which the best modern books are written, and to widen the mind by seeing the aspect

of science and affairs from a Continental standpoint. It is lamentable that there is so little professional intercourse between the students of one of our London Schools and the teachers of another. The laudable energy which has made each of them complete and well-equipped Colleges has had this drawback, that at the present day the attention of a diligent student is more confined to the teaching and practice of his own school than it was sixty or seventy years ago. The narrowness and prejudice bred by this isolation may be corrected by a visit to the famous sister Universities of Edinburgh or Dublin; for their complete removal no prescription is so efficient as a prolonged stay in Continental laboratories and hospitals. But even such a broad and liberal education, even familiarity with the daily advances of medical science recorded in periodicals and archives and year-books, or transmitted by telegraph to the wondering readers of the daily newspapers, is not all that is needful to make a learned Physician. We know well the difference between reading of an experiment, or even seeing it performed, and doing it with our own hands. We know the difference between studying a pathological atlas, or even a cabinet of histological slides, and seeing and handling morbid tissues and making sections for oneself. So also is there all the difference between learning the present conclusions as they stand recorded in the last edition of a text-book or compendium and tracing

the steps by which our present knowledge has been reached.

With regard, for instance, to the physiology of the circulation, it is not only curious but instructive to follow its gradual growth from Galen and Vesalius, Columbus, Cæsalpinus, and Servetus, to Harvey and Lower and Malpighi, to Hales and Vierordt, to Ludwig, and Chauveau, and Gaskell, and Roy. The only true scientific method is the historical one. If we only know the results of a science without the steps by which they have been reached, we have indeed its practical use, but lose half its educational value. We are almost in the position of an engineer who knows the conclusions of trigonometry by rote, but is ignorant of the demonstration. I would therefore urge upon junior Fellows, while still enjoying the prospect rather than the fruition of professional success, to spare some of the time which is unoccupied by work in wards and laboratories for the perusal of such antiquated works as have been published as much as twenty years ago, and particularly for gaining acquaintance at first hand with classics like Virchow's 'Cellular Pathology,' and the lectures of Watson, Trousseau, and Stokes; or, if their time and inclination does not allow of more extended researches, at least to read such succinct masterpieces as Laennec's 'Mediate Auscultation,' Heberden's 'Commentaries,' Sydenham's 'Treatise on Gout,' and Harvey on the 'Movement of the Heart and of the Blood.'

(iii) I would, moreover, exhort Fellows of the College to see that, while all the new methods of Experimental Pathology and of Pharmacology are carried out by duly trained physiologists, we do not neglect the fundamental method taught and practised by Harvey of inspecting the bodies of those who have died of disease. It was this union of Morbid Anatomy with Clinical Observation which made the discoveries of Laennec and of Bright really fruitful. Without these autopsies clinical medicine is but an empirical art, diagnosis a sham, and treatment little better than quackery. Too exclusive attention to Therapeutics is apt to bring a man dangerously near to Homœopathy and other Pretended Systems of treatment, but sound pathology, and diagnosis controlled by post-mortem inspection, give positive knowledge and that union of modest self-confidence and prudent enterprise which become the physician.

Lastly, I have to fulfil the duty of exhorting the Fellows of this ancient College "to continue in mutual love and affection" among ourselves; and this is the easiest task of all.

For if we must admit that Experimental Science in England, and particularly Scientific Pathology, is not surpassing our bygone achievements as it ought to surpass them, considering the increased number of competent labourers and the vastly improved methods of research; and if we admit that the

crowd of modern books and the distractions which we fondly imagine to be peculiar to our generation leave small opportunity for the cultivation of ancient learning; and if the prejudices of our patients, both gentle and simple, still make post-mortem inspections less common and systematic than they should be—whatever, I say, may be our shortcomings in these or in other respects, your Harveian Orator may honestly congratulate the College and the profession upon the concord and mutual esteem which has happily marked our history from the days of Linacre to those of Harvey, from the days of Arbuthnot and Garth to those of Meade and Freind, from the days of Fothergill and Heberden to those of Matthew Bailie, of Babington, and of Sir Thomas Watson.

Nor is it here alone that we may congratulate ourselves upon the liberal and cordial feelings which happily prevail. The same may, I believe, be said of our sister college, with which we are so happily united, not only in the necessary duties of examination, but in the nobler union of joint endeavours to search out the secrets of Nature by way of experiment. Long may this continue, for thereon depends not only the dignity and peace of our profession, but in great measure our power of doing good. Although our patients may sometimes find fault with what they call professional etiquette, the wiser among them know (and in the long run the wise lead the foolish) that this term really means the

observance of those rules which distinguish a profession from a trade, which make our calling honourable as well as honest, which check the arts of advertisement and direct our ambition to obtaining the suffrages, not of the public which *cannot*, but of our profession which *can*, judge truly—rules of conduct which are, in fact, nothing but the carrying into daily practice of the golden rule to do to others as we would they should do to us.

For maintaining and strengthening this spirit of concord and good feeling we depend upon each one of our Fellows, but especially on the example and authority of our Head—an example and authority which, as the College well knows, are worthily maintained by the untiring devotion to its best interests of our honoured President.

MEMOIR

ON

THE LIFE AND WORKS

OF

HARVEY

REPRINTED FROM THE LAST EDITION OF THE
'ENCYCLOPÆDIA BRITANNICA.'

William Harvey, the discoverer of the movements of the heart and the circulation of the blood, was the eldest son of Thomas Harvey, a prosperous Kentish yeoman, and was born at Folkestone, on April 15th, 1578.

After passing through the grammar school of Canterbury, when he had just entered his sixteenth year, he became a pensioner of Caius College, Cambridge (May, 1593). At nineteen he took his B.A. degree, and soon after, having chosen the profession of medicine, he went to study at Padua under Fabricius and Casserius. At this university, when twenty-four years old, Harvey became doctor of medicine, April, 1602.

He returned to England in the following year, the first of James I, and settled in London; where in 1604 he married the daughter of Dr. Lancelot Brown, who had been physician to Queen Elizabeth. In the same year Harvey became a candidate of the Royal College of Physicians, and was admitted a Fellow in June, 1607. In 1609 he obtained the reversion of the post of physician to St. Bartholomew's Hospital. His application was supported by the king himself and by Dr. Atkins, the president of the college. On the death of Dr. Wilkinson in the course of the same year, he succeeded to the post. He was thrice censor of the college, and in 1615 was appointed Lumleian lecturer.

In the following year—the year of Shakespeare's death —Harvey began his course of lectures, and first brought forward his views "upon the movements of the heart and blood." Meantime his practice increased, and he had the lord chancellor, Bacon, and the Earl of Arundel among his patients. In 1618 he was appointed physician extraordinary to James I, and on the next vacancy physician in ordinary to his successor. In 1628, the year of the publication of the "Exercitatio Anatomica de Motu Cordis

et Sanguinis," he was elected treasurer of the College of Physicians, but at the end of the following year he resigned the office, in order, by command of Charles I, to accompany the young Duke of Lennox (James Stewart, afterwards Duke of Richmond) on his travels. He appears to have revisited Italy, and returned in 1632. Four years later he accompanied the Earl of Arundel on his embassy to the emperor. He was eager in collecting objects of natural history, sometimes causing the earl anxiety for his safety by his excursions in a country infested by robbers after the Thirty Years' War. In a letter written on this journey, he says: "By-the-way, we could scarce see a dog, crow, kite, raven, or any bird, or anything to anatomize; only some few miserable people, the reliques of the war and the plague, whom famine had made anatomies before I came." Having returned to his practice in London at the close of the year 1636, he accompanied Charles I in one of his journeys to Scotland (1639 or 1641).

While at Edinburgh he probably visited the Bass Rock; for he minutely describes its abundant population of sea-fowl in his treatise 'De Generatione' (Exerc., xi, p. 221), and incidentally speaks of the then credited account of the solan goose growing on trees as a fable. He was in attendance on the king at the battle of Edgehill (October, 1642), where he withdrew under a hedge with the Prince of Wales and the Duke of York (then boys of twelve and ten years old), "and took out of his pocket a book and read. But he had not read very long before a bullet of a great gun grazed on the ground near him, which made him remove his station," as he afterwards told Aubrey. After the indecisive battle, Harvey followed Charles I to Oxford, "where," writes the same gossiping narrator, "I first saw him, but was then too young to be acquainted with so great a doctor. I remember he came several times to our college (Trinity) to George Bathurst, B.D., who had a hen to hatch eggs in his chamber, which they opened daily to see the

progress and way of generation." In Oxford he remained three years, and ran some risk of being superseded in his office at St. Bartholomew's Hospital, "because he hath withdrawn himself from his charge, and is retired to the party in arms against the Parliament." It was, no doubt, at this time that his lodgings at Whitehall were searched, and not only the furniture seized, but also invaluable manuscripts and anatomical preparations.

"Ignoscant mihi niveæ animæ, si, summarum injuriarum memor, levem gemitum effudero. Doloris mihi hæc causa est: cum, inter nuperos nostros tumultus et bella plusquam civilia, serenissimum regem (idque non solum senatûs permissione sed et jussu) sequor, rapaces quædam manus non modo ædium mearum supellectilem omnem expilarunt, sed etiam, quæ mihi causa gravior querimoniæ, adversaria mea, multorum annorum laboribus parta, e museo meo summoverunt.* Quo factum est ut observationes plurimæ, præsertim de generatione insectorum, cum reipublicæ literariæ (ausim dicere) detrimento, perierint."—'De Gen.,' Ex. lxviii.

To this loss Cowley refers:

> "O cursed war! who can forgive thee this?
> Houses and towns may rise again,
> And ten times easier 'tis
> To rebuild Paul's than any work of his."

While with the king at Oxford he was made warden of Merton College, but a year later, in 1646, that city surrendered to Fairfax, and Harvey returned to London. He was now sixty-eight years old, and, having resigned his appointments and relinquished the cares of practice, lived in learned retirement with one or other of his brothers.

* The College edition of 1766 has *summanarunt*, and so it stands in the first edition, published by Lud. Elzevir at Amsterdam in 1657. But this seems clearly wrong, and the conjectural emendation I have ventured to make has been approved by more learned friends.

It was in his brother Daniel's house at Combe that Dr. (afterwards Sir George) Ent, a faithful friend and disciple (1604—1689), visited him in 1650. "I found him," he says, "with a cheerful and sprightly countenance, investigating, like Democritus, the nature of things. Asking if all were well with him: 'How can that be,' he replied, 'when the state is so agitated with storms and I myself am yet in the open sea? And indeed, were not my mind solaced by my studies and the recollection of the observations I have formerly made, there is nothing which should make me desirous of a longer continuance. But thus employed, this obscure life and vacation from public cares, which would disgust other minds, is the medicine of mine.'"

The work on which he had been chiefly engaged at Oxford, and indeed since the publication of his Treatise on the Circulation in 1628, was an investigation into the recondite but deeply interesting subject of generation. Charles I, the enlightened patron of Harvey's studies, had put the royal deer parks at Windsor and Hampton Court at his disposal for this purpose, and had watched his demonstration of the growth of the chick with no less interest than the movements of the living heart. Harvey had now collected a large number of observations, though he would probably have delayed their publication. But Ent succeeded in obtaining the manuscripts, with authority to print them or not as he should find them. "I went from him," he says, "like another Jason in possession of the golden fleece, and when I came home and perused the pieces singly, I was amazed that so vast a treasure should have been so long hidden." The result was the publication of the 'Exercitationes de Generatione' (1651).

This was the last of Harvey's labours. He had now reached his seventy-third year. His theory of the circulation had been opposed and defended, and was now generally accepted by the most eminent anatomists both at home and abroad. He was known and honoured throughout Europe and the Royal College of Physicians

erected a statue in his honour in 1652, inscribed "viro monumentis suis immortali." In 1654 he was elected to the highest post in his profession, that of president of the same college; but the following day he met the assembled Fellows, and, declining the honour for himself on account of the infirmities of age, recommended the re-election of the late president Dr. Prujean. He accepted, however, the office of *consiliarius*, which he again held in the two following years. He had already enriched the college with other gifts beside the honour of his name. He had raised for them "a noble building of Roman architecture (rustic work with Corinthian pilasters), comprising a great parlour or conversation room below and a library above;" he had furnished the library with books, and filled the museum with "simples and rarities," as well as with specimens of instruments used in the surgical and obstetric branches of medicine. At last he determined to give to his beloved college his paternal estate at Burmarsh in Kent. His wife had died some years before, his brothers were wealthy men, and he was childless; so that he was defrauding no heir when, in July, 1656, he made the transfer to the college of this property, then valued at £56 per annum, with provision for a salary to the college librarian and for the endowment of an annual oration. The orator, so Harvey orders in his deed of gift, is to exhort the fellows of the college "to search out and study the secrets of nature by way of experiment, and also for the honour of the profession to continue mutual love and affection among themselves."

Harvey, like his contemporary and successor, Sydenham, was long afflicted with gout, but he preserved his activity of mind to an advanced age. In his eightieth year, on the 3rd of June, 1657,* he was attacked by hemiplegia, and though deprived of speech, was able to send for his

* This is the date usually given according to the college annals. Granger's 'Biographical History of England' makes it June 30th; Hamey, a contemporary, June 15th; while Dr. Lawrence, following

nephews, and distribute his watch, ring, and other personal trinkets among them. He died the same evening, "the palsy giving him an easy passport," and was buried, with great honour, in his brother Eliab's vault at Hempstead in Essex, "annorum et famæ satur."

Aubrey, to whom we owe most of the minor particulars about Harvey which have been preserved, says: "In person he was not tall, but of the lowest stature; round-faced, olivaster complexion, little eyes—round, very black—full of spirits; his hair black as a raven, but quite white twenty years before he died." The best portrait of him extant is by Cornelius Jansen, in the library of the College of Physicians, one of those rescued from the great fire, which destroyed their original hall in 1666. It has been often engraved, and is prefixed to the fine edition of his works, published in 1766. A second contemporary portrait, taken probably by another Dutch artist, is preserved, with those of his father and brothers, at Roll's Park, near Chigwell, once the seat of his eldest brother, Eliab Harvey.

Harvey's Work on the Circulation.—In estimating the character and value of the discovery announced in the 'Exercitatio de Motu Cordis et Sanguinis,' it is necessary to bear in mind the previous state of knowledge on the subject. Aristotle taught that in man and the higher animals the blood was elaborated from the food in the liver, whence it was carried to the heart, and sent through the veins over the body. His successors of the Alexandrian school of medicine, Erasistratus and Herophilus, further elaborated his system, and taught that, while the veins carried blood from the heart to the members, the arteries carried a subtle kind of air or spirit. For the practical physician only two changes had been made in this theory of the circulation between the Christian era and the six-

the inscription on the tomb, gives June 3rd, 1658—a mistake of a year.

teenth century. Galen had discovered that the arteries were not, as their name implies, merely air-pipes, but that they contained blood as well as vital air or spirit. And it had been gradually ascertained that the nerves (*νεῦρα*) which arose from the brain and conveyed "animal spirits" to the body were different from the tendons or sinews (*νεῦρα*) which attach muscles to bones.*

First, then, the physicians of the time of Linacre (*ob.* 1524) knew that the blood is not stagnant in the body. So did Shakespeare and Homer, and every augur who inspected the entrails of a victim, and every village barber who breathed a vein. Plato even uses the expression *τὸ αἷμα κατὰ πάντα τὰ μελὴ σφοδρῶς περιφερέσθαι.* But no one had a conception of a continuous stream returning to its source (a circulation in the true sense of the word), either in the system or in the lungs. If they used the word *circulatio,* as did Cæsalpinus,† it was as vaguely as the French policeman cries "Circulez" to the crowd. The movements of the blood were thought to be slow, and irregular in direction as well as in speed, like the "circulation" of air in a house, or the circulation of traffic in the streets of a city. *Secondly,* they supposed that one kind of blood flowed from the liver to the right ventricle of the heart, and thence to the lungs and the general system by the veins; and that another kind flowed from the left ventricle to the lungs and general system by the arteries. *Thirdly,* they supposed that the septum of the heart was pervious, and allowed blood to pass directly from the right to the left side. *Fourthly,* they had no conception of the functions of the heart as the motor power of the movement of the blood. They doubted whether its substance was muscular; they

* It is to the latter sort of "nerves" that the adjective "nervous" applies when used tropically of a "nervous" style of writing.

† Indeed, the same word, *περίοδος αἵματος,* occurs in the Hippocratic writings, and was held by van der Linden to prove that to the father of medicine himself, and not to Columbus or Cæsalpinus, belonged the laurels of Harvey.

supposed its pulsation to be due to expansion of the spirits it contained; they believed the only dynamic effect which it had on the blood to be sucking it in during its active diastole, and they supposed the chief use of its constant movements to be the due mixture of blood and spirits.

Of the great anatomists of the sixteenth century, Jacobus Sylvius ('In Hipp. et Gal. physiologiæ partem anatom. isagoge,' 1555) described the valves of the veins.

Vesalius ('De Humani Corporis Fabricâ,' 1542) ascertained that the septum between the right and left ventricles is complete, though he could not bring himself to deny the invisible pores which Galen's system demanded.

Servetus, in his 'Christianismi Restitutio' (1553), goes somewhat further than his fellow-student Vesalius, and says: *Paries ille medius non est aptus ad communicationem et elaborationem illam; licet aliquid resudare possit.* From this doubtfully expressed anatomical fact, and the large size of the pulmonary arteries, he concludes that there is a communication in the lungs by which blood passes from the pulmonary artery to the pulmonary vein: *eodem artificio quo in hepate fit transfusio a vena portæ ad venam cavam propter sanguinem, fit etiam in pulmone transfusio a vena arteriosa ad arteriam venosam propter spiritum.* The natural spirit of the left side and the vital spirit of the right side of the heart were therefore, he concluded, practically the same, and hence two (the vital and the animal) instead of three distinct *spiritus* should be admitted. It seems doubtful whether even Servetus rightly conceived of the entire mass of the blood passing through the pulmonary artery and the lungs The transference of the *spiritus naturalis* to the lungs, and its return to the left ventricle as *spiritus vitalis,* was the function which he regarded as important. Indeed, a true conception of the lesser circulation as a transference of the whole blood of the right side to the left was impossible until the corresponding transference in the greater or systemic circulation was discovered. Servetus, however, was the true predecessor of Harvey in

physiology, and his claims to that honour are perfectly authentic and universally admitted.

The way, then, to Harvey's great work had been paved by the discovery of the valves in the veins, and by that of the lesser circulation—the former due to Sylvius and Fabricius, the latter to Servetus; but the significance of the valves was unsuspected, and the fact of even the pulmonary circulation was not generally known, at least in its full meaning.

Realdus Columbus ('De Re Anatomica,' 1559) formally denies the muscularity of the heart, yet correctly teaches that blood and spirits pass from the right to the left ventricle, not through the septum, but through the lungs; and adds, probably in ignorance of Servetus' priority, *quod nemo hactenus aut animadvertit aut scriptum reliquit.* The fact that Harvey quotes Columbus, and not Servetus, is explained by the almost entire destruction of the writings of the latter, which are now among the rarest curiosities.*

The great anatomist, Hieronymus Fabricius (*ob.* 1619), Harvey's teacher at Padua, described the valves of the veins more perfectly than had Sylvius.

Ruini, in his treatise on the 'Anatomy and Diseases of the Horse' (1590), taught that the left ventricle sends blood and vital spirits to all parts of the body except the lungs—the ordinary Galenical doctrine. Yet on the strength of this phrase a tablet has actually been put up in the veterinary school at Bologna, in memory of Ruini as the discoverer of the circulation of the blood!

The claims of Cæsalpinus to Harvey's laurels, though more plausible, are scarcely better founded. In his 'Quæstiones Peripateticæ' (1571) he followed Servetus and Columbus in describing what we now know as "the pulmonary part of the circulation" under that name, and

* The works of Servetus, as well as their author, had been burnt in 1553, just a quarter of a century before Harvey was born. The *Christianismi Resitutio* thus became excessively scarce, and it is very unlikely that Harvey ever saw a copy.

this is the only foundation for the assertion (first made in Bayle's dictionary) that Cæsalpinus knew "the circulation of the blood." He is even behind Servetus, for he only allows part of the blood of the right ventricle to go round by this "circuit;" some, he conceives, passes through the hypothetical pores in the septum, and the rest by the superior cava to the head and arms, by the inferior to the rest of the body—*Hanc esse venarum utilitatem ut omnes partes corporis sanguinem pro nutrimento deferant: ex dextro ventriculo cordis vena cava sanguinem crassiorem, in quo calor intensus est magis; ex altero autem ventriculo sanguinem temperatissimum ac sincerissimum habente, egreditur aorta.* Cæsalpinus seems to have had no original views on the subject; all that he writes is copied from Galen or from Servetus except some erroneous observations of his own. His greatest merit was as a botanist. No claim to the "discovery of the circulation" was made by him or by his contemporaries on his behalf, and when it was made, Haller decided conclusively against it. The fact that an inscription was lately placed on the bust of Cæsalpinus at Rome, which states that he preceded others in recognizing and demonstrating "the general circulation of the blood," is only a proof of the blindness of misplaced national vanity.

In his treatise Harvey proves (1) that it is the contraction, not the dilatation, of the heart which coincides with the pulse, and that the ventricles as true muscular sacs squeeze the blood which they contain into the aorta and pulmonary artery; (2) that the pulse is not produced by the arteries enlarging and so filling, but by the arteries being filled with blood and so enlarging; (3) that there are no pores in the septum of the heart, so that the whole blood in the right ventricle is sent to the lungs and round by the pulmonary veins to the left ventricle, and also that the whole blood in the left ventricle is again sent into the arteries, round by the smaller veins into the venæ cavæ, and by them to the right ventricle again—thus making a

complete double "circulation"; (4) that the blood in the arteries and that in the veins is the same blood; (5) that the action of the right and left sides of the heart, auricles, ventricles, and valves, is the same, the mechanism in both being for reception and propulsion of liquid and not of air, since the blood on the left side, though mixed with air, is still blood; (6) that the blood sent through the arteries to the tissues is not all used, but that most of it runs through into the veins; (7) that there is no to-and-fro undulation in the veins, but a constant stream from the distant parts towards the heart; (8) that the dynamical starting-point of the blood is the heart and not the liver.

The method by which Harvey arrived at his complete and almost faultless solution of the most fundamental and difficult problem in physiology has been often discussed, and is well worthy of attention. He begins his treatise by pointing out the many inconsistencies and defects in the Galenical theory, quoting the writings of Galen himself, of Fabricius, Columbus, and others, with great respect, but with unflinching criticism. For, in his own noble language, wise men must learn anatomy, not from the decrees of philosophers, but from the fabric of nature herself, *nec ita in verba jurare antiquitatis magistræ, ut veritatem amicam in apertis relinquant, et in conspectu omnium deserant.* He had, as we know, not only furnished himself with all the knowledge that books and the instructions of the best anatomists of Italy could give, but, by a long series of dissections, had gained a far more complete knowledge of the comparative anatomy of the heart and vessels than any contemporary—we may almost say than any successor until the times of Hunter and Meckel. Thus equipped, he tells us that he began his investigations into the movements of the heart and blood by looking at them, *i. e.* by seeing their action in living animals. After a modest preface he heads his first chapter "*Ex vivorum dissectione qualis sit cordis motus.*" He minutely describes what he saw and handled in dogs, pigs, serpents, frogs,

and fishes, and even in slugs, oysters, lobsters, and insects, in the transparent *minima squilla*, "quæ Anglice dicitur, *a shrimp*," and lastly in the chick while still in the shell. In these investigations he used a *perspicillum*, or simple lens. He particularly describes his observations and experiments on the ventricles, the auricles, the arteries, and the veins. He shows how the arrangement of the vessels in the fœtus supports his theory. He adduces facts observed in disease as well as in health to prove the rapidity of the circulation. He explains how the mechanism of the valves in the veins is adapted, not, as Fabricius believed, to moderate the flow of blood *from* the heart, but to favour its flow *to* the heart. He estimates the capacity of each ventricle, and reckons the rate at which the whole mass of blood passes through it. He elaborately and clearly demonstrates the effect of obstruction of the blood-stream in arteries or in veins by the forceps in the case of a snake, by a ligature on the arm of a man, and illustrates his argument by figures. He then sums up his conclusion thus: *Circulari quodam motu, in circuitu, agitari in animalibus sanguinem, et esse in perpetuo motu; et hanc esse actionem sive functionem cordis quam pulsu peragit; et omnino motus et pulsus cordis causam unam esse.* Lastly, in the fifteenth, sixteenth, and seventeenth chapters, he adds certain confirmatory evidence, as the effect of position on the circulation, the absorption of animal poisons and of medicines applied externally, the muscular structure of the heart, and the necessary working of its valves. The whole treatise, which occupies only sixty-seven pages of large print in the quarto edition of 1766, is a model of accurate observation, patient accumulation of facts, ingenious experimentation, bold yet cautious hypothesis, and logical deduction.

In one point only was the demonstration of the circulation incomplete. Harvey could not discover the capillary channels by which the blood passes from the arteries to the veins. This gap in the circulation was supplied several years later by the great Italian anatomist Malpighi, who,

in 1661, saw in the lungs of a frog, by help of the newly-invented microscope, how the blood passes from the one set of vessels to the other. Harvey saw all that could then be seen in his observations on living animals; Malpighi, four years after Harvey's death, by another observation on a living animal, completed the splendid chain of evidence. If this detracts from Harvey's merit, it leaves Servetus no merit at all. But, in fact, the existence of the channels, first seen by Malpighi, was as clearly pointed to by Harvey's reasoning as the existence of Neptune by the calculations of Le Verrier and of Adams.*

Harvey himself and all his contemporaries were well aware of the novelty and importance of his theory. He says, in the admirable letter to Dr Argent, president of the College of Physicians, which follows the dedication of his treatise to Charles I, that he should not have ventured to publish "a book which alone asserts that the blood pursues its course and flows back again by a new path, contrary to the received doctrine taught so many ages by innumerable learned and illustrious men," if he had not set forth his theory for more than nine years in his college lectures, gradually brought it to perfection, and convinced his colleagues by actual demonstrations of the truth of what he advanced. He anticipates opposition, and even obloquy or loss, from the novelty of his views. These anticipations, however, the event proved to have been groundless. If we are to credit Aubrey, indeed, he found that after the publication of the 'De Motu,' "he

* The following extract from a letter shows the sort of counter-claims to Harvey's discovery which were made in the seventeenth century:

Isaac Walton to John Aubrey, December 2nd, 1680.

"Mr Warner did long and constantly lodge near the water stairs or market in Woolstable (not far from Charing Cross). My Lord of Winchester tells me he knew him, and that he said he first found out the circulation of the blood and discovered it to Dr Harvey (who said that 'twas he himself that found it), for which he is so memorably famous."

fell mightily in his practice; 'twas believed by the vulgar that he was crackbrained, and all the physicians were against him." But the last assertion is demonstrably untrue; and if apothecaries and patients ever forsook him they must soon have returned, for Harvey left a handsome fortune. By his own profession the book was received as it deserved. So novel a doctrine was not to be accepted without due inquiry, but his colleagues had heard his lectures and seen his demonstrations for years; they were already convinced of the truth of his theory, urged its publication, continued him in his lectureship, and paid him every honour in their power.

Abroad the book was widely read and much canvassed. Few accepted the new theory; but no one dreamt of claiming the honour of it for himself, nor for several years did anyone pretend that it could be found in the works of previous authors. The first attack on it was a feeble tract by James Primerose,* a pupil of Riolanus ('Exerc. et Animadv. in librum Harvei de Motu Cord. et Sang.,' 1630). Five years later Parisanus, an Italian physician, published his 'Lapis Lydius de Motu Cord. et Sang' (Venice, 1635), a still more bulky and futile performance. Primerose's attacks were, according to the contemporary judgment of the excellent anatomist Johann Vesling, of Padua, *imbellia pleraque* and *sine ictu;* that of Parisanus, *in quamplurimis turpius.* Their dulness has protected them from further censure.

Caspar Hoffmann, professor at Nuremberg, while admitting the truth of the lesser circulation in the full Harveian sense, denied the rest of the new doctrine. To him the English anatomist replied in a short letter, still extant, with great consideration, yet with modest dignity, beseeching him to convince himself by actual inspection of the truth of the facts in question. He concludes: "I accept your censure in the candid and friendly spirit in which you say you wrote it; do you also the

* Perhaps a son of Gilbert Primrose, surgeon to James VI of Scotland.

same to me, now that I have answered you in the same spirit." This letter is dated May, 1636, and in that year Harvey passed through Nuremberg with the Earl of Arundel, and visited Hoffmann. But he failed to convince him. *Nec tamen valuit Harveius vel coram,* writes Schlegel, who, however, afterwards succeeded in persuading the obstinate old Galenist to soften his opposition to the new doctrine, and thinks that his complete conversion might have been effected if he had but lived a little longer—*nec dubito quin concessisset tandem in nostra castra.*

While in Italy, the following year, Harvey visited his old University of Padua, and demonstrated his views to Professor Vesling. A few months later this anatomist wrote him a courteous and sensible letter, with certain objections to the new theory. The answer to this has not been preserved, but it convinced his candid opponent, who admitted the truth of the circulation in a second letter (both were published in 1640), and afterwards told a friend, *Harveium nostrum si audis, agnosces cœlatum sanguinis et spiritus ingressum ex arteriis per venas in dextrum cordis sinum.*

Meanwhile a greater convert, Descartes, in his 'Discours sur la Méthode' (1637), had announced his adhesion to the new doctrine, and refers to "the English physician to whom belongs the honour of having first shown that the course of the blood in the body is nothing less than a kind of perpetual movement in a circle." Walæus of Leyden, Regius of Utrecht, and Schlegel of Hamburg, successively adopted the new physiology. Of these professors, Regius mauled the pertinacious Primerose, and was mauled by him in return ('Spongia qua eluuntur sordes quæ Jac. Primirosius,' etc., and 'Antidotum adv. Spongiam venenatam Henr. Regii'). Descartes afterwards repeated Harvey's vivisections, and, more convinced than ever, demolished an unlucky Professor Plempius, of Louvain, who had written on the other side. Dr George Ent also published an 'Apologia pro Circulatione Sanguinis,' in answer to Parisanus.

At last Riolan ventured to publish his 'Enchiridium Anatomicum' (1648), in which he attacks Harvey's theory, and proposes one of his own. Riolan had formerly accompanied the queen dowager of France (the widow of Henri IV) on a visit to her daughter at Whitehall, and had there met Harvey and discussed his theory. He was, in the opinion of the judicious Haller, *vir asper et in nuperos suosque cœvos immitis ac nemini parcens, nimis avidus suarum laudum præco, et se ipso fatente anatomicorum princeps.* Harvey replied to the 'Enchiridium' with perfectly courteous language and perfectly conclusive arguments, in two letters —'De Circulatione Sanguinis'—which were published at Cambridge in 1649, and are still well worth reading. He speaks here of the *circuitus sanguinis, a me inventus.* Riolan was unconvinced, but lived to see another professor of anatomy appointed in his own university, who taught Harvey's doctrines. Even in Italy, Trullius, professor of anatomy at Rome, expounded the new doctrine in 1651.

The most fruitful adherents of the new doctrine were Pecquet, of Dieppe, the discoverer of the thoracic duct and of the true course of the lacteal vessels, and Thomas Bartholinus, of Copenhagen, in his 'Anatome ex omnium Veterum Recentiorumque Observationibus, imprimis Institutionibus beati mei parentis Caspari Bartholini, ad Circulationem Harveianam et vasa lymphatica renovata' (Leyden, 1651). At last Plempius also retracted all his objections, for, as he candidly stated, "having opened the bodies of a few living dogs, I find that all Harvey's statements are perfectly true." Hobbes, of Malmesbury, could thus say in the preface to his "Elementa Philosophiæ" that his friend Harvey *solus quod sciam, doctrinam novam superatâ invidiâ vivens stabilivit.*

It has been made a reproach to Harvey that he failed to appreciate the importance of the discoveries of the lacteal and lymphatic vessels by Aselli, Pecquet, and Bartholinus. In three letters on the subject—one to Dr Morrison, of Paris (1652), and two to Dr Horst, of Darm-

stadt (1655), a correspondent of Bartholin's, he discusses these observations, and shows himself unconvinced of their accuracy. He writes, however, with moderation and reasonableness, and excuses himself from investigating the subject further on the score of the infirmities of age (he was then above seventy-four). The following quotation shows the spirit of these letters:—*Laudo equidem summopere Pecqueti aliorumque in indagandâ veritate industriam singularem, nec dubito quin multa adhuc in Democriti puteo abscondita sint, a venturi sæculi indefatigabili diligentiâ expromenda.* Bartholin, though reasonably disappointed in not having Harvey's concurrence, speaks of him with the utmost respect, and generously says that the glory of discovering the movements of the heart and of the blood was enough for one man.

Harvey's Work on Generation.—We have seen how Dr Ent persuaded his friend to publish this book in 1651. It is between five and six times as long as the 'Exerc. de Motu Cord. et Sang.,' and is followed by three 'Excursus: de Partu, de Uteri Membranis, de Conceptione.' Though the fruit of as patient and extensive observations, its value is far inferior to that of the earlier treatise. The subject was more abstruse, and, in fact, inaccessible to proper investigation without the aid of the microscope; and the field was almost untrodden since the days of Aristotle. Fabricius, Harvey's master, in his work, 'De formatione Ovi et Pulli' (1621), had alone preceded him in modern times. Moreover, the seventy-two chapters which form the book lack the co-ordination so conspicuous in the earlier treatise, and some of them seem almost like detached chapters of a system which was never completed or finally revised.

Aristotle had believed that the male parent furnished the body of the future embryo, while the female only nourished and formed it.* Galen taught almost as

* This is, in fact, the theory on which, in the 'Eumenides' of Æschylus, Apollo obtains the acquittal of Orestes.

erroneously that each parent contributes seeds, the union of which produces the young animal. Harvey, after speaking with due honour of Aristotle and Fabricius, begins rightly "ab ovo;" for, as he remarks, "Eggs cost little, and are always and everywhere to be had," and, moreover, "almost all animals, even those which bring forth their young alive, and man himself, are produced from eggs" (*omnia omnino animalia, etiam vivipara atque hominem adeo ipsum, ex ovo progigni*). This dictum, usually quoted as *omne vivum ex ovo*, would alone stamp this work as worthy of the discoverer of the circulation of the blood, but it was a prevision of genius, and was not proved to be a fact until von Baer discovered the mammalian ovum in 1827.

Harvey proceeds with a careful anatomical description of the ovary and oviduct of the hen; he describes the new-laid egg, and gives an account of the appearance seen on the successive days of incubation, from the first to the sixth, the tenth, and the fourteenth, and, lastly, he gives an account of the process of hatching. He then comments upon and corrects the opinions of Aristotle and Fabricius, declares against spontaneous generation (though in one passage he seems to admit the current doctrine of production of worms by putrefaction as an exception), proves that there is no *semen fœmineum*, that the chalazæ of the hen's egg are not the *semen galli*, and that both parents contribute to the formation of the egg. He accurately describes the first appearance of the ovarian ova as mere specks, their assumption of yelk, and afterwards of albumen. In chapter xlv he describes two methods of production of the embryo from the ovum—one is *metamorphosis*, or the direct transformation of pre-existing material, as a worm from an egg, or a butterfly from an *aurelia* (chrysalis); the other is *epigenesis*, or development with addition of parts, the true generation observed in all higher animals.*

* The two methods nearly correspond with the "evolution" and the "epigenesis" which were discussed in the early part of the present century as exclusive theories of development. Even now,

Chapters xlvi–l are devoted to the abstruse question of the efficient cause of generation, which, after much discussion of the opinions of Aristotle and of Sennertus, Harvey refers to the action of both parents, as the instruments of the first great Cause.*

He next goes on to describe the order in which the several parts appear in the chick. He states that the *punctum saliens,* or fœtal heart, is the first organ to be seen, and explains that the nutrition of the chick is not only effected by yelk conveyed directly into the midgut, as Aristotle taught, but also by absorption from yelk and white by the umbilical (omphalomeseraic) veins; on the fourth day of incubation appear two masses (which he oddly names *vermiculus*), one of which develops into three vesicles, to form the cerebrum, cerebellum, and eyes, the other into the breastbone and thorax; on the sixth or seventh day come the viscera; and lastly, the feathers and other external parts.

Harvey remarks how nearly this order of development in the chick agrees with what he had observed in mammalian and particularly in human embryos. He notes the bifid apex of the fœtal heart in man, and the equal thickness of the ventricles, the soft cartilages

the controversy between the views of Weissmann and of Hertwig ranges over much the same ground.

* "Superior itaque et divinior opifex, quam est homo, videtur hominem fabricare et conservare, et nobilior artifex, quam gallus pullum ex ovo producere. Nempe agnoscimus Deum, creatorem summum atque omnipotentem, in cunctorum animalium fabricâ ubique præsentem esse, et in operibus suis quasi digito monstrari: cujus in procreatione pulli instrumenta sint gallus et gallina. . . . Nec cuiquam sane hæc attributa conveniunt nisi omnipotenti rerum Principio, quocunque demum nomine idipsum appellare libuerit sive Mentem divinam cum Aristotele, sive cum Platone Animam Mundi, aut cum aliis Naturam naturantem, vel cum ethnicis Saturnum aut Iovem; vel potius (ut nos decet) Creatorem ac Patrem omnium quæ in cœlis et terris, a quo animalia eorumque origines dependent, cujusque nutu sive effato fiunt et generantur omnia."—Exerc. liv.

which represent the future bones, the large amount of liquor amnii, and absence of placenta which characterise the fœtus in the third month; in the fourth the position of the testes in the abdomen, and the uterus with its Fallopian tubes resembling the uterus bicornis of the sheep; the large thymus; the cæcum, small as in the adult, not forming a second stomach, as in the pig, the horse, and the hare; the lobulated kidneys, like those of the seal ("*vitulo*"—*subanditur marino*) and porpoise, and the large suprarenal veins, not much smaller than those of the kidneys (li–lvi). He failed, however, to trace the connexion of the urachus with the bladder.

In the following chapters (lxiii–lxxii) he describes the process of generation in the fallow deer and the roe. After again insisting that all animals arise from ova, that a "conception" is an internal egg and an egg an extruded conception, he goes on to describe the uterus of the doe, the process of impregnation, and the subsequent development of the fœtus and its membranes, the *punctum saliens*, the cotyledons of the placenta, and the "uterine milk," to which Professor Turner has recalled attention. The treatise concludes with detached notes on the placenta, parturition, and allied subjects.

Harvey's other Writings and Medical Practice.—The remaining writings of Harvey which are extant are unimportant. A complete list of them will be found below, together with the titles of those which we know to be lost. Of these the most important were probably the tractate on respiration, and the records of post-mortem examinations. It seems that he had a notion of respiration being connected rather with production of animal heat than—as then generally supposed—with cooling of the blood.*

* Hæc qui diligenter perpenderit, naturamque aëris diligenter introspexerit, facile opinor fatebitur eundem nec refrigerationis gratiâ nec in pabulum animalibus concedi. Hæc autem obiter duntaxat de respiratione diximus, proprio loco de eadem forsitan copiosius disceptaturi.—'De Partu,' p. 550.

Of Harvey as a practising physician we know very little. Aubrey tells us that "he paid his visits on horseback with a footcloth, his man following on foot, as the fashion then was." He adds: "Though all of his profession would allow him to be an excellent anatomist, I never heard any that admired his therapeutic way. I knew several practitioners that would not have given threepence for one of his bills" (the apothecaries used to collect physicians' prescriptions and sell or publish them to their own profit), "and that a man could hardly tell by his bill what he did aim at." However this may have been—and rational therapeutics was impossible when the foundation-stone of physiology had only just been laid—we know that Harvey was an active practitioner, performing such important surgical operations as the removal of a breast, and he turned his obstetric experience to account in his book on generation. Some good practical precepts as to the conduct of labour are quoted by Willoughby, a contemporary. He also took notes of the anatomy of disease; these unfortunately perished with his other manuscripts, otherwise we might regard him as a forerunner of Morgagni; for Harvey saw that pathology is but a branch of physiology, and, like it, must depend first on accurate anatomy. He speaks strongly to this purpose in his first epistle to Riolanus: *Sicut enim sanorum et boni habitûs corporum dissectio plurimum ad philosophiam et rectam physiologiam facit, ita corporum morbosorum et cachecticorum inspectio potissimum ad pathologiam philosophicam.* The only specimen we have of his observations in morbid anatomy is his account of the post-mortem examination made by order of the king on the body of the famous Thomas Parr, who died in 1635, at the reputed age of 152.

Harvey insists on the value of physiological truths for their own sake, independently of their immediate utility; but he himself gives us an interesting example of the practical application of his theory of the circulation in the cure

of a large tumour by tying the arteries which supplied it with blood ('De Generat.,' Exerc. xix).

Dr Wray, of Spilsby, Lincolnshire, sent (May, 1880) to the medical journals the following extract, from the same treatise, as a contribution to the history of intrauterine therapeutics:

"The wife of a Doctor of Divinity, who was of a good habit of body enough, but being barren, did consult me; and being very desirous of children, she had tried many medicines and physitians, but all to no purpose; she had her termes at the usual times, but sometimes (especially when she had rode on horseback) some corrupt and purulent substance did issue from her, which presently after would stop again. Some conceived it to be the whites; others suspected it to be some deep ulcer, being persuaded thereunto chiefly because her flux was not constant and by little and little, but by certain intervals, and much at a time. Whereupon, by the help of a *speculum matricis*, they did survey all the sheath of the uterus [*vagina*], and did apply several medicines, but all in vain. At last I, being called, did open the inward orifice of the womb, and presently there did issue forth to the quantity of two spoonfuls of corrupt matter, sprinkled with bloody streaks; which, when I perceived, I told them that there lay an ulcer lurking in the cavity of her womb; and by injecting proper medicaments I restored her to her former health. But being intent upon the cure, and seeing the ordinary remedies did little availe, I applied more forcible ones, because I suspected that the ulcer was inveterate (and perhaps with flesh growing upon it), wherefore to my former injections I added a little Roman vitriol, by whose acrimony the uterus being stimulated, did grow so hard, that it did seem as hard to the touch as a stone, and occasioned several hysterical symptomes, which physitians commonly conceive to proceed from the suffocation of the matrix, and foul vapours being thus sent upwards. This inconvenience continued awhile, till the uterus being assuaged by milder applica-

tions, and such as abate pains, did relax its orifice again and did exclude the sharp liquour which I had injected, together with a putrid matter, whereby the patient was in a short time restored."

The following is believed to be a complete list of all the known writings of Harvey, published and unpublished:

'Exercitatio Anatomica de Motu Cordis et Sanguinis,' 4to, Frankfort-on-the-Maine, 1628;* 'Exercitationes duæ Anatomicæ de Circulatione Sanguinis, ad Johannem Riolanum, filium, Parisiensem,' Cambridge, 1649; 'Exercitationes de Generatione Animalium, quibus accedunt quædam de Partu, de Membranis ac Humoribus Uteri, et de Conceptione,' 4to, Lond., 1651; 'Anatomia Thomæ Parr,' first published in the treatise of Dr John Betts, 'De Ortu et Natura Sanguinis,' 8vo, Lond., 1669.

Letters: (1) To Caspar Hoffmann, of Nuremberg, May, 1636; (2) to Schlegel, of Hamburg, April, 1651; (3) three to Giovanni Nardi, of Florence, July, 1651, Dec., 1653, and Nov., 1655; (4) one to Dr Morrison, of Paris, May, 1652; (5) two to Dr Horst, of Darmstadt, Feb., 1654–5, and July, 1655; (6) to Dr Vlackveld, of Haarlem, May, 1657. His letters to Hoffmann and Schlegel are on the circulation; those to Morrison, Horst, and Vlackveld refer to the discovery of the lacteals; the two to Nardi are short letters of friendship. All these letters were published by Sir George Ent in his collected works (Leyden, 1687). Of two MS. letters, one on official business to the secretary, Lord Dorchester, was printed by Dr Aveling, with a facsimile of the crabbed handwriting ('Memorials of Harvey,' 1875), and the other, about a patient, appears in Dr Willis's 'Life of Harvey,' 1878.

'Prælectiones anatomiæ universalis per me Gul. Harveium medicum Londinensem, anat. et chir. professorem, an. dom. 1616, ætat. 37'—MS. notes of his Lumleian

* Reproduced in facsimile with a translation into English by Morton: Canterbury, 1895.

lectures in Latin—are in the British Museum library; they are almost illegible, but were partly deciphered and a photograph of one of the pages taken by Sir Edward Sieveking. A second MS. has been discovered in the British Museum, entitled 'Gulielmus Harveius de Musculis, Motu Locali,' etc., and an account of it was published by Dr George Paget ('Notice of an unpublished MS. of Harvey,' Lond., 1850).*

The following treatises, or notes towards them, were lost either in the pillaging of Harvey's house, or perhaps in the fire of London, which destroyed the old College of Physicians:—'A Treatise on Respiration,' promised, and probably, at least in part, completed (pp. 82, 550, ed. 1766); 'Observationes de usu Lienis;' 'Observationes de motu locali,' perhaps identical with the above-mentioned manuscript; 'Tractatum physiologicum;' 'Anatomia medicalis' (apparently notes of morbid anatomy); 'De Generatione Insectorum.'

The fine 4to edition of Harvey's 'Works,' published by the Royal College of Physicians in 1766, was superintended by Dr Mark Akenside; it contains the two treatises, the two Epistles to Riolanus, the account of the post-mortem examination of old Parr, Harvey's diploma, and the nine letters mentioned above. A translation of this volume by Dr Willis, with Harvey's will, was published by the Sydenham Society, 8vo, Lond., 1849.

The 'Prælectiones Anatomiæ Universalis' (1616), above mentioned, were reproduced in facsimile, with each page deciphered and printed; and were published by subscription, accompanied by a printed version, in 1886.

The following are the principal biographies of Harvey:—in Aubrey's 'Letters of Eminent Persons, etc.' (vol. ii, London, 1813), first published in 1685, the only contemporary

* My learned friend Dr Payne informs me that this MS. is apparently another part of the series of lectures, and contains other subjects than those in the volume of 1616 which is now published. It is dated 1627.

account; in Bayle's 'Dictionnaire Historique et Critique,' 1698 and 1720 (Eng. ed. 1738); in the 'Biographia Britannica,' and in Aiken's 'Biographical Memoirs;' the Latin Life by Dr Lawrence, prefixed to the college edition of 'Harvey's Works' in 1766; Memoir in 'Lives of British Physicians,' Lond., 1830; a Life by Dr Robert Willis, founded on that by Lawrence, and prefixed to his English edition of Harvey in 1847; the much enlarged Life by the same author, published in 1878; the biography by Dr Munk in the 'Roll of the College of Physicians,' 2nd ed., vol. i, 1879, and that by Dr Norman Moore, in the 'Dictionary of National Biography.'*

The literature which has arisen on the great discovery of Harvey, on his methods and his merits, would fill a library. The most important contemporary writings have been mentioned above. The following list gives some of the more remarkable in modern times:—the article in Bayle's Dictionary, quoted above; 'Anatomical Lectures,' by Wm. Hunter, M.D., 1774; Sprengel, 'Geschichte der Arzneikunde,' Halle, 1800, vol. iv; Flourens, 'Histoire de la Circulation,' 1854; Lewes, 'Physiology of Common Life,' 1859, vol. i, pp. 291-345; Ceradini, 'La Scoperta della Circolazione del Sangue,' Milan, 1876; Tollin, 'Die Entdeckung des Blutkreislaufs durch Michael Servet,' Jena, 1876; Kirchner, 'Die Entdeckung des Blutkreislaufs,' Berlin, 1878; Willis, in his Life of Harvey; Wharton Jones, 'Lecture on the Circulation of the Blood,' published in the 'Lancet' for October 25 and November 1, 1879; and the 'Harveian Orations,' especially those by Sir Edward Sieveking, Dr Guy, Professor Rolleston, and Dr Stone.

* The last has appeared since the present memoir was published.

APPENDIX

Synopsis of Known Conditions of Disease in General (Page 7)

I. *External, exciting reaction of the tissues:*

Mechanical injury—irritant or destructive.
Electrical disturbance—irritant or destructive.
Chemical irritants (poisons—drugs—food).
Heat and cold—irritant or destructive.
Indirect action of heat and of cold
Deficiency of food—depressant.
Deficiency of oxygen—depressant.
Invasion of parasites—irritant or destructive.
Toxic absorption—irritant—depressant—febrile.
Contagion.
Climate.

II. *Internal, predisposing conditions:*

Race (as separable from climate).
Occupation—work—strain.
Sex.
Age.
Slight functional disorders becoming permanent by repetition.
Chronic diseases preparing for acute.
Congenital malformations.
Lesions due to intra-uterine diseases.
Hereditary diseases.
Hereditary predispositions.
Degeneration and wear.
Proclivity, vulnerability, resistance by habit, immunity.

Incidence of Diseases of the Nervous System upon Men and Women
(Pages 62 and 99–107)

	M.	F.			
Apoplexy (cerebral hæmorrhage)	82	to 31	or	2·7 : 1	(Guy's).
Cerebral Tumours	27	to 15	or	1·8 : 1	(P.-S.).
Ditto	67	to 33	or	2 : 1	(Hale White).
Cerebral abscess	44	to 12	or	4 : 1	(Pitt).
Tuberculous meningitis	80	to 37	or	2 : 1	(Fagge).
Acute myelitis	19	to 6	or	3 : 1	(P.-S.).
Progressive muscular atrophy	176	to 33	or	5 : 1	(Friedreich).
Ditto	84	to 15	or	5·5 : 1	(Sir Wm. Roberts).
Bulbar paralysis	34	to 19	or	2 : 1	(Kussmaul).
Pseudo-hypertrophic paralysis	25	to 14	or	2 : 1	(Guy's).
Ditto	41	to 5	or	8 : 1	(Webber, U.S.A.).
Ditto	33	to 10	or	3 : 1	(Gowers).
Tabes dorsalis	75	to 8	or	9 : 1	(Guy's).
Ditto	.	.	.	8 : 1	(Erb).
Chorea	159	to 423	or	1 : 2·5	(Guy's, P.-S.).
Sciatica	.	.	.	4 : 1	(Erb).

Eight Tables of the Relative Frequency of Chorea in Male and Female Subjects

	M.	F.
Hughes (Guy's—1846)	27	73
„ („ 1855)	42	167
Pye-Smith (Guy's)	159	423
Sée (H. des Enfans malades)	138	393
Hillier (Ormond Street)	122	300
Goodhart (Evelina)	43	98
Wincke (Leipzig)	117	210
Brit. Med. Assoc. (Collective Investigation)	115	322

Totals 763 to 1986 or 1 : 2·6.

Incidence of Diseases in General on the Sexes

Men. *Women.*

= Infective Fevers =

..... Whooping Cough.

= Infantile Palsy =

Progressive muscular atrophy and bulbar paralysis.
Pseudo-hypertrophic atrophy.
Tabes

= Friedreich's Disease =

= Insular Sclerosis =

Cerebral abscess and tumours.
General paralysis of the insane.
Tetanilla.
Thomsen's disease.
Tetanus.

..... Megrim.

Myxœdema, 15 : 94. 'Clin. Soc. Report.'

Bronchocele.

Laryngismus str., 2 : 1.

= Bronchitis =

Plastic bronchitis.
Pneumonia and Pleurisy. .....

= Phthisis =

Hay fever.
Asthma, 2 : 1.
Angina pectoris, 10 : 1 .
Aortic valvular disease. Mitral valvular disease, especially mitral stenosis.
..... Rheumatic pericarditis.
Aneurysm.

Exophthalmic goitre.

(Von Graefe, 6 : 1. Trousseau, 6¼ : 1. Henoch, *6* : 1.)

= Dyspepsia =

..... Gastric Ulcer.
Carcinoma ventriculi, 2 : 1. .....
Typhlitis, 24 : 9 (P.-S.).
Invagination (in children), about 2 : 1.
Strangulation of bowel.
„ by diverticulum ilei, almost exclusively in males.

Men. *Women.*

= Hydatids =

Tuberculous peritonitis ..... Gall-stones.

Cirrhosis of liver, 102 : 16. Acute yellow atrophy, 55 : 88 (Thierfelder).

Diabetes insipidus.

Hæmoglobinuria.

Renal calculus, 6 : 1 Moveable kidney, 10 : 87 (Landau). 8 : 92 (Steffler).

Diabetes.

2 : 1 (Guy's). 2·45 : 1 (Pavy, private cases).

M. Addisonii, 119 : 64 (Greenhow).

Leuchæmia, 2 : 1 Chlorosis.

Anæmia lymphatica, 3 : 1 (Gowers).

= Addison's Idiopathic Anæmia. =

cf. p. 68

Hæmophilia, 12 : 1 (Wickham Legg).

Gout.

Rheumatic fever, 223 : 177 (P.-S.).

In children under 14, 27 : 42 (Goodhart).

Gonorrhœal synovitis, 105 : 7 (P.-S.).

Osteo-arthritis ..... Osteo-arthritis

(later cases in hip). (early cases in hands).

= Rickets =

Mollities ossium, 13 : 132 (Durham).

..... Cancer.

cf. pp. 61, 62, and footnote.

Senile gangrene.

= Eczema =

= Psoriasis =

145 : 122 (P.-S.)

Pemphigus, 1 : 3 (P.-S.).

Erythema, 59 : 39.

= Acne =

= Tineæ =

Area? 72 : 40 (P.-S.)

Leucoma linguæ et oris. Sclerodermia, 3 : 19 (P.-S.).

Xanthelasma, 1 : 2.

= Lupus =

Zona, 61 : 39 (P.-S.).

INCIDENCE ON MEN AND WOMEN OF "IDIOPATHIC ANÆMIA" OF ADDISON (1855), "ESSENTIAL ANÆMIA" OF LEBERT (1858), "PROGRESSIVE PERNICIOUS ANÆMIA" OF BIERMER (1868)
(Page 67)

Hermann Müller's collected cases from Zürich (1877):

Men	9
Women	35

Eichhorst's collected cases (1878):

Men	30;	primary	11
Women	65	„	12

Dr Coupland's collected cases (1881):

Men	56
Women	54

Cases in Guy's Hospital (1859—1889):

Men	21
Women	15

Collected cases, with autopsy ('Guy's Hosp. Rep.,' 1882):

Men	46
Women	56

Dr Musser's cases in America:

Men	24
Women	15

Table illustrating the Incidence of Death from Certain Diseases upon Male and Female Subjects

(*From the Registrar-General's Returns*)

(Pages 59—66)

Date.	Disease.	Male.	Female.
1848–80	Ague	2,609	2,111 =
„	Erysipelas	35,308	32,177 =
„	Syphilis	24,015	22,099 =
1858–85	"Rheumatism" and rheumatic morbus cordis	19,937	18,804 =
1848–80	Gout	9,681	2,572 M.
1850–85	Diabetes	15,922	9,178 M.
1848–80	Cancer	84,913	187,734 F.
„	Phthisis	827,394	874,633 =
„	Chorea	624	1,614 F.
„	Apoplexy	168,719	171,949 =
„	Aneurysm	12,374	3,975 M.
„	Pneumonia	440,238	339,846 M.
„	Bronchitis	617,103	597,977 =
„	Asthma	72,336	53,182 M.
„	Peritonitis	22,544	32,013 F.
„	Hernia	15,159	14,448 =
1858–85	Gall-stones	950	2,054 F.
1848–80	Calculus	6,123	811 M.

Table of Diseases as they affect Different Ages
(Page 69)

Diseases of Infants under Two Years old:

Diarrhœa and vomiting. Marasmus.
Ulcerative colitis (infantile dysentery).
Whooping-cough.
Diphtheria.
Lobular pneumonia.
Invagination of bowel.
Tuberculosis (especially meningitis and peritonitis).
Hydrocephalus.
Rickets. Convulsions. Tetany.
Strabismus.
Laryngismus stridulus.
Acute atrophic paralysis.
Eczema.
Molluscum contagiosum.

Diseases of Later Childhood—from Two to Fifteen Years:

Varicella.
Measles.
Rubeola.
Scarlatina.
Whooping-cough.
Mumps.
Croup (spasmodic laryngitis).
Diphtheria.
Rheumatic peri- and endo-carditis.
Empyema.
Typhlitis.
Tuberculous tumours of brain (especially of cerebellum).
Caries of spine.
Pseudo-hypertrophic paralysis.
Epilepsy.
Chorea.
Stammering.
Spasmodic asthma.
Impetigo.
Lupus.
Ringworm.

Diseases of Puberty—Fourteen to Eighteen Years:

Acne.
Anæmia and chlorosis.
Hysteria.

Diseases of Adult Life—Twenty to Fifty Years:

Phthisis.
Syphilis.
Rheumatic fever (early adult life).
Gout (late adult life).
Dyspepsia.
Enteric fever.
Typhlitis (early adult life).
Morbus cordis of rheumatic origin.
Sick headache.
Neuralgia.
Tabes.
Insular sclerosis.
General paralysis of the insane.
Lupus erythematosus.

Diseases of Later Life—Fifty to Seventy Years:

Carcinoma.
Gout.
Angina pectoris.
Osteo-arthritis.
Morbus cordis of atheromatous origin.
Aneurysm.
Apoplexy (cerebral hæmorrhage).
Bronchitis.
Diabetes.
Interstitial nephritis.

Table of Endemic Diseases
(Page 70)

Plague.—Kurdestan, and parts of Persia, Tripoli, Arabia, and Southern China.
Cholera.—Bengal.
Typhus.—China and Tartary (?).
Yellow fever.—West Coast of Africa (now epidemic in Tropical America).
Dengue.—West Indies, East Indies.
Kakké (*Beri-beri*).—Japan, Ceylon, &c.
Yaws.—West Africa and West Indies.
Blackwater fever.—West Africa.
Maltese fever.—Gibraltar and the Mediterranean.
Malarial fevers.—Mediterranean and tropics.
Dysentery.—Malarial regions.
Abscess of liver.—India and China.
Hydatids.—Australia, Iceland.
Elephantiasis.—West Indies, Cape, Egypt, India, Polynesia.
Leprosy.—Norway, Orient, India, China, West Indies, &c.
Bronchocele.—Derbyshire, the European Alps, &c.
Calculi (uric acid).—East Anglia.
Cretinism.—Alpine valleys.

A Suggestion of Geographical Regions from a Pathological Point of View
(Page 71)

Arctic regions.—Iceland, Greenland, Siberia, Canada.
North temperate.—Northern and Central Europe, Mediterranean. Arabia and Persia—Tartary and Thibet—China and Japan. United States.
Tropical.—India and East India Islands—West Indies, Mexico, and Spanish Main—Central Africa—North Australia.
South temperate.—Australia, Cape Colony and Natal, Argentina and Chili.
Mountainous countries with deep valleys.
Oceanic islands.—New Zealand, Polynesia, Madeira and Canaries, &c.

Table of the Seasonal Distribution of Acute Rheumatism

(Pages 72 and 85)

London, ten years (P.-S.).		London, nine years (Gabbett)		Paris, four years (Besnier)	
Sept.	158	Nov.	248	July	861
—		—		—	
Nov.	136	Oct.	215	June	797
Jan.	127	Dec.	201	April	781
April	127	—		—	
Oct.	127	Sept.	174	May	751
—		July	171	—	
June	111	Jan.	165	Aug.	729
Aug.	112	Aug.	159	Dec.	721
May	108	June	156	Nov.	701
Dec.	102	—		Oct.	701
July	100	May	130	—	
Feb.	99	April	127	Feb.	669
—		Feb.	132	March	666
March	69	March	122	Sept.	661
				—	
				Jan.	593
	1376		2000		8631

*** The latter two tables are quoted from Dr. Archibald Garrod.

Statistics of the Relation between Chorea and Rheumatism

(Page 116)

In	58	cases of	chorea	(Hughes—Guy's)	17	of precedent	rheumatism.
,,	104	,,	,,	(B. Brown ,,)	89	,,	,,
,,	150	,,	,,	(Manser ,,)	42	,,	,,
,,	163	,,	,,	(Halstead ,,)	53	,,	,,
,,	128	,,	,,	(Sée)	61	,,	,,
,,	100	,,	,,	(Sturges)	20	,,	,,
,,	104	,,	,,	(Donkin)	27	,,	,,
,,	130	,,	,,	(Goodhart)	89	,,	,,
,,	172	,,	,,	(Mackenzie)	47	,,	,,
,,	439	,,	,,	(Br. Med. Assoc.)	116	,,	,,
,,	80	,,	,,	(Herringham)	21	,,	,,
,,	80	,,	,,	(A. E. Garrod)	35	,,	,,

Gastric Ulcer

(Pages 66 and 137)

Clinical Cases:

Age.	Male.	Female.	Totals.
Under 9	0	0	0
10—19	2	9	11
20—29	6	24	30
30—39	9	14	23
40—49	12	10	22
50—59	4	3	7
	33	60	93

Post-mortem Cases:

Age.	Male.	Female.	Totals.
Under 9	2	2	4
10—19	3	4	7
20–29	7	5	12
30—39	7	8	15
40—49	15	7	22
50—59	15	15	30
60—66	9	0	9
70—72	1	1	2
	59	42	101

Abscess of Liver

86 *Cases*

	Single.	Multiple.	Totals.
Injury to ribs	2	0	2
Extension from lung, stomach, or gall-bladder	4	5	9
Suppurating hydatid	6	3	9
Suppurating bile-ducts (hepatic pyæmia)	0	6	6
Dysentery, typhlitis, &c. (intestinal pyæmia)	11	18	29
Perimetritis, &c. (pelvic pyæmia)	2	5	7
Ulcerative endocarditis, &c. (general pyæmia)	3	16	19
Actinomycosis	1	1	2
Tropical abscess	3	0	3
	32	54	= 86

ÆTIOLOGY OF SPECIFIC INFECTIOUS DISEASES CERTAINLY OR WITH MORE OR LESS PROBABILITY ASCRIBED TO MICROPHYTES

(Page 18)

Anthrax	Bacillus	Davaine.
Relapsing fever	Spirillum	Obermeier.
Tuberculosis	Bacillus	Koch.
Glanders	,,	Löffler.
Leprosy	,,	Hansen.
Diphtheria	,,	Löffler.
Enterica	,,	Eberth.
Cholera	Vibrio	Koch.
Pneumonia	Diplococcus	Fränkel.
Erysipelas	Streptococcus	Fehleisen.
Tetanus	Bacillus	Kitasato.
Plague	,,	,,
Influenza	,,	Pfeiffer.

The four which are placed at the head of the list fulfil all the conditions of rigorous proof, and the four which follow them are nearly as certainly established.

(1) The disease is specific and definite.

(2) The microbe is specific, and can also be certainly identified in its size and form, or its staining qualities, or its mode of reproduction, or the form it assumes when cultivated in various soils.

(3) It is constantly present in the blood or lymph or tissues (not only in the skin or epithelium of mucous membrane).

(4) It is never present except in cases of the disease.

(5) When isolated in a pure cultivation, and introduced into the organism of a man or mammal, it reproduces (*a*) itself in multitudes, and (*b*) the symptoms of the disease.

Maternal Impressions

(Page 37)

From Dr Rennie's Report at Fowchow

From year to year several cases of difficult labour among natives come under my care, and although the subject of the illustration did not and could not increase the difficulties, it was the appearance of a headless infant, whose head was supposed to be retained, that caused the native midwife to seek my assistance.

On visiting the mother I found that after an illness of four hours' duration, and half an hour before my visit, she had given birth to an acephalous monster. The placenta had been expelled, the womb had contracted, and all that was left for me to do was to assure the mother that her labour was ended.

The mother was twenty-two years of age, had been married for seven years, and had previously given birth to two healthy children, now aged three and five years respectively.

The infant was said to have moved and to have emitted respiratory sounds after birth. The body was well developed, the hands were clubbed, and the feet were in the condition of equino-varus. Unlike most others of the same class, it was not a twin.

Local folk-lore attributes the cause of this deformity to the mother, during gestation, sitting at night before a lamp and using a pair of scissors. The shadow of the scissors while in use is supposed to penetrate the womb and cause the deformity in the fœtus. Harelip and intra-uterine amputations of fœtal limbs are also attributed to the same cause.—*Medical Reports from Chinese ports published by the Inspector-General of Customs in China for the year ended 31st March*, 1891.

Cerebral Abscess

(Page 68)

Of the 56 cases examined by Dr Pitt, 44 occurred in men and 12 in women. In 51 of these the abscess was due to—

Pyæmia . .	in 7 men	and 1 woman.
Traumatic . .	9	1
Ear disease . .	11	7
Secondary to lung .	7	1
Remaining cases .	6	1

Antecedents of Lardaceous Disease

(Page 145)

Of 244 cases of lardaceous disease published by the late Dr Hilton Fagge in the 'Path. Trans.,' 1876 (vol. xxvii, p. 334), 193 were preceded by prolonged suppuration, 67 of these were cases of phthisis, 36 of syphilis, and 51 of caries. In 45 other cases there was evidence of syphilis without suppuration.

Of 302 cases collected for the present writer by Dr H. J. Campbell, also from Guy's Hospital, and consecutive to those of Dr Fagge, there had been prolonged suppuration in 124, and in addition 123 of phthisis, and 43 of syphilis.

Combining the two series of cases, which thus extend from 1854 to 1889 inclusive, we find that of 546 consecutive cases of lardaceous disease observed at the same hospital there were—

Cases of phthisis	190
Cases of syphilis	124
Other cases of prolonged suppuration, mostly of bones	214
Unaccounted for	18
	546

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